American Political Science Review

MARCH 1995
VOLUME 89 NUMBER 1

MANAGING EDITOR

G. Bingham Powell, Jr.
Department of Political Science
University of Rochester
Rochester, New York 14627-0148

BOOK REVIEW EDITOR

Melissa P. Collie
Department of Government
Burdine Hall 536
University of Texas at Austin
Austin, Texas 78712-1087

EDITORIAL BOARD

John H. Aldrich
Duke University
James E. Alt
Harvard University
Henry E. Brady
University of California, Berkeley
Valerie Bunce
Cornell University
Gary W. Cox
University of California, San Diego
Russell Dalton
University of California, Irvine
Lee Epstein
Washington University
James Farr
University of Minnesota
James L. Gibson
University of Houston
Michael A. Gillespie
Duke University
Joanne Gowa
Princeton University
John W. Kingdon
University of Michigan
Michael Laver
Trinity College, Dublin
Jack Levy
Rutgers University
Gary J. Miller
Washington University
Dianne M. Pinderhughes
University of Illinois, Urbana
Paul J. Quirk
University of Illinois, Urbana
Kay L. Schlozman
Boston College
Mary L. Shanley
Vassar College
Susan Shirk
University of California, San Diego
Barbara Sinclair
University of California, Riverside
Harvey Starr
University of South Carolina

EDITORIAL STAFF

Linda F. Lindenfelser
Associate Editor
Timothy I. M. Fackler
Nathalie J. Frensley
John J. Janssen
Assistants to the
Book Review Editor
Michael K. Lane
Copy Editor
Jay Goodliffe
Nathan Dietz
Peter Stone
APSR Interns
Thomas P. Keating
Editorial Assistant

The *American Political Science Review* (ISSN-0003-0554) appears quarterly. It is published by the American Political Science Association, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036, and sent to all members. Dues: Regular Members with gross calendar income: under $30,000, $55; $30,000–$40,000, $75; $41,000–$50,000, $85; $51,000–$60,000, $90; over $60,000, $95; Student Members (limited to 5 years), $25; Retired Members (who have been members 25 years) with gross calendar income; under $25,000, $20; $25,000 and over, $40; Life Members, $2,000; Institutional Members: Domestic, $147; Foreign, $165. Dues allocated for a subscription to the *APSR*: $17 for individual members; $110 for institutional members. Changes of address sent to Membership Secretary of the APSA. Postmaster: Send address changes to: APSR, 1527 New Hampshire Ave., N.W., Washington, D.C. 20036. Second-class postage paid at Washington, D.C. and at additional mailing offices. Copyright © 1995 by the *American Political Science Association.*

American
Political
Science
Review

Table of Contents

ARTICLES

A Way of Life and Law
Charles O. Jones 1

The Concept of a Critical Realignment, Electoral Behavior, and Political Change
Peter F. Nardulli 10

Party Cleavages and Welfare Effort in the American States
Robert D. Brown 23

Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections
Jorge I. Domínguez and James A. McCann 34

Modeling Negative Campaigning
Stergios Skaperdas and Bernard Grofman 49

Political Control Versus Expertise: Congressional Choices about Administrative Procedures
Kathleen Bawn 62

Choosing the Best Social Order: New Principles of Justice and Normative Dimensions of Choice
Grzegorz Lissowski and Piotr Swistak 74

RESEARCH NOTES

Economic and Referendum Voting: A Comparison of Gubernatorial and Senatorial Elections
Lonna Rae Atkeson and Randall W. Partin 99

Disentangling Patterns of State Debt Financing
James C. Clingermayer and B. Dan Wood 108

Avarice and Ambition in Congress: Representatives' Decisions to Run or Retire from the U.S. House
Richard L. Hall and Robert Van Houweling 121

Condorcet Winners and the Paradox of Voting:
Probability Calculations for Weak Preference Orders
Bradford Jones, Benjamin Radcliff, Charles Taber, and Richard Timpone 137

CONTROVERSIES

Civic Virtue and Self-Interest
Christopher M. Duncan; Shelley Burtt 147

Aristotle's Science of the Best Regime
Mary P. Nichols; Robert C. Bartlett 152

NOTES FROM THE MANAGING EDITOR 161

BOOK REVIEWS

Review Essays:

The Civil Culture at Thirty
David D. Laitin 168

New Horizons in Institutional Analysis
Elinor Ostrom 174

Opinion and Representation
James Stimson 179

Political Theory 184

American Politics 191

Comparative Politics 217

International Relations 240

QUARTERLY JOURNAL OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

MARCH 1995

VOLUME 89 NUMBER 1

American Political Science Association

1527 New Hampshire Avenue,
Washington, D.C. 20036

PRESIDENT:
Sidney Verba
Harvard University

PRESIDENT ELECT:
Arend Lijphart
University of California, San Diego

VICE PRESIDENTS:
F. Chris Garcia
University of New Mexico
Betty Glad
University of South Carolina
Catherine Kelleher
U.S. Mission to NATO

SECRETARY:
Loch Johnson
University of Georgia

TREASURER:
Susan Borque
Smith College

EXECUTIVE DIRECTOR:
Catherine E. Rudder

CO-PROGRAM CHAIRS:
Mary Katzenstein
Cornell University
Peter Katzenstein
Cornell University

MANAGING EDITOR, APSR:
G. Bingham Powell, Jr.
University of Rochester

COUNCIL, 1993–95:
John A. Garcia
University of Arizona
Barbara Geddes
University of California, Los Angeles
John Mark Hansen
University of Chicago
Anne H. Hopkins
University of Minnesota
Margaret Levi
University of Washington
Mamie E. Locke
Hampton University
William E. Nelson, Jr.
The Ohio State University
Michael P. Zuckert
Carleton College

COUNCIL, 1994–96:
Timothy Cook
Williams College
Susan MacManus
University of South Florida
Helen Milner
Columbia University
Mary Nichols
Fordham University
David Price
Duke University
Theda Skocpol
Harvard University
Toni-Michelle C. Travis
George Mason University
Eddie Williams
*Joint Center for Political and
Economic Studies*

INSTRUCTIONS TO CONTRIBUTORS

The *American Political Science Review* aims to publish scholarly research and writing of exceptional merit. Contributions must demonstrate the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors must demonstrate how their analysis or exposition illuminates a significant research problem, or answers an important research question, of general interest in political science.

Article Manuscripts

Article manuscripts should be submitted to the Managing Editor, Professor G. Bingham Powell, Jr., *American Political Science Review*, Department of Political Science, University of Rochester, Rochester, New York 14627-0148. Four copies must be submitted, and none can be returned. Manuscripts must be typed double-spaced, on one side of the paper only, and include an abstract of no more than 150 words. The author's name and affiliation should not appear on the copies submitted, but only on a separate covering sheet. In general, the *Review* considers manuscripts of up to 30 pages in length. Moreover, the *Review* does not consider manuscripts submitted to other journals or to a publisher as part of a larger manuscript.

Manuscripts should be prepared following the guidelines of the *Style Manual for Political Science*, a publication of the American Political Science Association's Committee on Publications. It is available from the Association's office. Otherwise, authors should follow the *Chicago Style Manual* and the example of recent issues of the *Review*. Graphs and tables should be presented on separate pages, with their location in the manuscript indicated, e.g., the notification "Table 1 about here," at the proper places.

Book Reviews

All book review correspondence should be sent to the Book Review Editor, Professor Mark Lichbach, *American Political Science Review* Book Review, Department of Political Science, University of Colorado, Box 333, Boulder, CO 80309. Authors wishing their book to be considered for review should ask their publisher to send a copy to the Book Review Editor. Those interested in reviewing books should send their vita to the Book Review Editor.

Other Correspondence

Information, including news and notes, for the Association's news journal *PS*, should be sent to Dr. Robert J-P. Hauck, Editor, American Political Science Association, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. Advertising, reprint, and circulation correspondence should be sent to the Executive Director at the Washington office. Domestic claims for nonreceipt of issues must be made within four months of the month of publication; overseas claims, eight months. Advertising information and rates are available from Diana Falls, Advertising Manager, APSA, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.

Indexing

Articles and notes appearing in the *Review* before the June 1953 issue were indexed in *The Reader's Guide to Periodical Literature*. Current issues are indexed by *International Political Science Abstracts*, *United States Political Science Documents*, and *Social Sciences Index*. Microfilm of the *Review*, beginning with Volume 1, may be obtained from University Microfilms, 300 North Zeeb Road, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review*, Volumes 1–62, 1906–1968, also may be obtained from University Microfilms. Articles appearing in the *Review* are listed in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences*. Book Reviews are indexed in *Book Review Index*.

ARTICLES

American Political Science Review Vol. 89, No. 1 March 1995

A WAY OF LIFE AND LAW

Presidential Address, American Political Science Association, 1994

CHARLES O. JONES

This address stresses the importance of the study of lawmaking. Three advantages in particular are emphasized: lawmaking is the core decision-making process in a democracy, its study offers an opportunity for Americanists to overcome concentrations on a single institution, and it provides a basis for comparative analysis. The discussion focuses on statute making as a primary phase of lawmaking. Four concepts—iteration, inquiry, speculation, and declaration—are identified as key, unexplored characteristics of statute making that hold substantial promise for research.

> The American way is first a way of life and then of law—and then of life as the final object of government.
> —T.V. Smith, *The Legislative Way of Life*

Otto von Bismarck is believed to have said, "If you like laws and sausages, you should never watch either one being made." It is an observation widely cited by political cynics. I agree when it comes to the making of sausages. However, law is not bratwurst. I cannot accept that we should never watch laws being made. Such an aphorism is a threat to my professional raison d'être, as well as to that of many of my closest friends. Even more important—and not unrelated—to decline to observe lawmaking is to deny oneself an understanding of how democracy works, how life is expressed through law. I would not have expected that point to have troubled Bismarck. But it should bother political scholars greatly.

Therefore I will use this occasion to invite more concentrated study of lawmaking. As understood here, lawmaking is the means by which governments legitimize substantive and procedural actions to reshape public problems, perhaps to resolve them. Law is a major defining and stabilizing component for subsequent policy decisions, so that we may witness a revisiting of intent and meaning as specific laws and authoritative judgments are implemented and evaluated. Lawmaking for any one issue is a trackable process as legislators, executives, bureaucrats, judges, and others variably participate in statute making, rule-and-standard setting, administrative and executive interpretation, and court decision making, which, combined, constitute the legitimate base for public policy. The authorizing processes of lawmaking are therefore interactive with the initiating and executing processes of policymaking.

The justification for studying lawmaking, so conceived, is threefold:

1. Lawmaking is the core decision-making process of a democratic state. It is the means for defining, promoting, and regulating community life and, accordingly, is spectacularly interesting and highly relevant to our purposes as political scientists.
2. It offers the opportunity to integrate research, because the study of lawmaking cannot be confined to one institution, not even the legislature.
3. It affords a basis for comparative and historical analysis that is lacking in segmented institutional study. As a further contribution to comparative analysis, study of lawmaking in the United States reinforces the separationist over the presidentialist nature of the political system.

In this address, I shall review these advantages of a research focus on lawmaking, suggest concepts that hold promise for the comparative study of statute making as one major facet of the lawmaking enterprise, and briefly discuss tests of the lawmaking capacity of democratic states.

THE CORE PROCESS

In his classic work, *The Growth of American Law*, J. Willard Hurst explained, "Law moves with the main currents of American history" (1950, 13). Again,

> The most creative, driving, and powerful pressures upon our law emerged from the social setting. Social environment has two aspects: First, it is what [people] think: how they size up the universe and their place in it; what things they value, and how much; what they believe to be the relations between cause and effect, and the way these ideas affect their notions of how to go about getting the things they value. Second, it is what [people] do: their habits, their institutions. Ideas and institutions obviously are inseparable aspects of . . . history. We do not have to try to decide which, if either, is the more powerful. (p. 11)

T. V. Smith, a man who knew both the academic and political worlds, expressed this view in reflecting on legislative life: "Blessed is the man who, through patience, sagacity, and co-operation with other men, can take the purely private thing called conscience and turn it into socially acceptable action through law. . . . Whoever has a genius for legislation has a mission of major importance to mankind" (1940, 72–73).

Hurst and Smith make lawmaking sound vital and engrossing, interactive with social life and the human experience. Yet it has received limited direct attention by political scientists, even by legislative scholars.[1] Martin Shapiro finds this deficiency rather curious: "Given the incredibly detailed and sophisticated levels that congressional studies have reached in political science, the refusal to pay any attention at all to the substance of the laws that Congress makes seems almost willfully perverse" (1989, 3:101). He is just as critical of his colleagues in public law: "If Congress specialists are so afraid of law that they will not read statutes, we might expect public law specialists to help them out. . . . But theories of law emanating from public law specialists are built up almost entirely from their observations of judge-made constitutional law rather than all law" (ibid.).

Why is this subject not of greater interest to us? Perhaps because the study of law is bound to be issue-specific and will be marked by the dreaded label *case study,* with its limited potential for theory. But, of course, there is no rule that one must study the making of just one law any more than there is an edict that one must study just one committee or one election or one legislature (a not uncommon practice, by the way). And, in any event, are we to conclude that a study that is right and limited has less merit for its scope than one that is wrong and broadly applied? I think not. Let us not allow methodological fashions or conventions to determine either what we study or what we are permitted to learn from the work of others. In that same spirit, let us not fail to study a vital subject because of an estimate of how the work will be received or because of a judgment in advance that the research will be difficult. We are free to tackle the important political questions. Truly, we are obliged to do so.

Can the case study tag, with all that it implies, be overcome? There is no guarantee as long as one has decided to examine the actual work of government. But there are means by which scholars may build sufficient confidence as fortification against the patronizing review of their work as "just a case study." The first advisory is, do it right. Select an issue because of its public importance and conceptual interest. Alas, even doing it right may not guarantee publication in our prestige journals. An expert on health policy and politics surely contributes as much to basic and applied political science as an expert on the high incumbent-return rates of non-check-kiting members of the House of Representatives or the continuity of party voting in presidential elections. Yet I invite you to compare the number of published articles in our mainline journals on the former versus the latter.[2] The second advisory is related to the first: work from a conceptual framework that permits integration of findings so that study of one issue, like one election, can contribute to a broader understanding of government and politics. Realizing this goal requires the identification of concepts that facilitate a substantive or issue focus in studying the lawmaking process (as contrasted to an organizational or behavioral focus) yet are promising for comparison among political systems. I will introduce and discuss several such concepts.

Is there reason either to overcome the case study tag or to gain enough confidence to ignore it? The answer is an unqualified *yes.* As Hurst and Smith have led us to expect, lawmaking is so interesting—so absolutely captivating—that studying it will give you a reason to get up in the morning. It will put bounce back in your step. Lawmaking is life made public: How can a *social* scientist resist observing how that happens? Further, it surely is a part of our job description as political scientists. We study politics, presumably because it interests us. To be interested in politics is to care about government and lawmaking. Our subjects and methods vary, but even the normal scientists among us have selected politics as an object of attention.

We tend to study institutional processes and political behavior more than policy substance. Yet governments make laws to alter public problems, maybe even to solve some of them. To watch government work is to be struck by the attention paid to substance—to those issues currently on the agenda and those programs already on the books. These issues and programs have a forceful physical presence in the designation of buildings in a capital (transportation, education, health and human services, labor) and in the committees of a legislature (agriculture, education, finance, energy). In visiting an agency, or sitting in on an executive staff meeting, or listening to a committee hearing, or observing a representative meet with constituents, one cannot miss the dominance of public issues as central to the talk and work of government. There is, of course, good reason to study and generalize about the organizational features of institutions and the social and political behavior of legislators and bureaucrats. But there is also a strong justification for relating that behavior to the content of issues, which, through representation, reflect the ways of life.

THE OPPORTUNITY FOR CROSS-INSTITUTIONAL STUDY

The second advantage of a concentration on lawmaking is that it offers a means for cross-institutional analysis—a special benefit for Americanists. It is acknowledged that the branches of government are often mixed up in each other's business. Yet it is typical in the classroom to offer courses on the presidency, Congress, the courts, the bureaucracy, political parties, and interest groups. This pattern extends to graduate study, where students often specialize in one of the institutions; to hiring, with searches often identified as being for a "Congress," "presidency," or "judicial" scholar; and to research, as well, with publications accommodating these specializations even to the point of having journals devoted primarily to one institution or its work. With the development of organized sections of the American Political Science Association, it was to be expected that there would be sections on legislative studies, presidency research, public administration, political organization and parties, and law, courts, and judicial process.

One can hardly begrudge these developments. We have profited greatly from this apportionment, with scholars laying claim to an institution and then proceeding through a lifetime of productive research to inform us as to how the place works. And given the coequality of the branches in a separated system, one can perhaps understand why it is that scholars tend to choose one or the other for concentrated study. There are, however, risks associated with this division of labor—risks related to our understanding that the separation is ultimately a means for governing and risks related to "going native" by identifying too closely with our institution of choice and overestimating its role in governing.

In regard to the first risk, it is worth remembering that the Founders were seeking to devise a working government. They did, of course, have fears about tyranny and thereby sought protection through competing legitimacies. But the point was not solely to stop the bad from happening; it was to permit the good, or even the middling, to occur as well. And so, as students of American government—not just one branch thereof—we need to be mindful that it is difficult to discern and weigh achievement with a method that is exclusively segmental.

The second risk is related to the first. It is natural to become attached to that with which we become identified. A commonly acknowledged problem is the failure then to remain objective, the developing of a bias in favor of the institution one has come to possess—even perhaps to define for others. A less-featured issue is the problem caused by a failure to relate the work of your institution to that of others with which it is inexorably associated and to which it is sometimes dependent. The result may be a distorted or flawed understanding of the institution of choice.

A focus on lawmaking ensures that the scholars will exceed the bounds of any one institution. In its fullest sense (and by the original design in a separated system), lawmaking is a cross-institutional, often interlevel, enterprise. Not even statute making, which is most closely associated with the legislature, can conceivably be studied only within that body. Thus the substance of lawmaking forces scholars of the separated system out of one institution even at the early stages of decision making. Perhaps that is why there has been limited study of lawmaking by political scientists, while justifying the need for that work to be done.

THE COMPARATIVE ADVANTAGE

A third advantage of the concentrated study of lawmaking is its potential in comparative study. Political systems vary greatly in organization and their institutional relationships, yet all make law. The trick is to identify means and develop concepts that permit meaningful comparisons. That goal is unlikely to be achieved by comparing one institution with another (e.g., the British House of Commons with the American House of Representatives, the Norwegian Storting with the South Korean National Assembly). Such comparisons typically reaffirm the differences in the structure of the basic system but are of more limited utility for understanding lawmaking.

The problem of comparative analysis from an institutional base is particularly acute for Americanists, who are rightly criticized for their parochialism in failing to make comparisons with other systems. In part, this negligence may be an effect of the segmentation of institutional study noted earlier. Congressional scholars, for example, may well convince themselves that Congress is sui generis; presidency and Supreme Court scholars may do the same. Many scholars, therefore, justify becoming one-country, one-institution specialists, lacking any incentive to make comparisons. The more they burrow in, the less likely they are to compare, particularly since the institutional emphasis makes comparison of limited value. Why bother, if the effort is not satisfying or is unproductive?

Perhaps, too, Americanists are confused by how it is that comparativists classify the United States among the world's democracies. Presidentialism and parliamentarism are the categories relied on most.[3] The United States is often identified as "the model and prototype of presidential government," yet it is commonly acknowledged that "presidential government is often associated with the theory of the separation of powers" (Verney 1992, 40, 38). Why the limited-term president should win the label over a high-incumbent-return Congress, a career bureaucracy, and a life-term court is not altogether obvious.

The classification scheme—presidentialism and parliamentarism—creates problems that are avoidable. The United States has a separated system, not a presidential system. It is widely accepted that the separation of powers is a defining mark of the American government. It features coequal branches, sometimes sharing powers, sometimes competing for shares of power. In accepting this outstanding feature of the American system, scholars interested in lawmaking should be inspired to formulate cross-institutional concepts that may then serve well for cross-national comparisons. In other words, the separated nature of the American system invites segmented institutional study, which, in turn, complicates comparative analysis with the more unified one-party presidential systems and parliamentary systems. Therefore, a strictly institutional basis of comparison is not only of limited value, it probably cannot be accomplished due to the tendency of Americanists to pick off only one of the separated institutions for concentrated study. The study of lawmaking, like that of policymaking, enables scholars to break free of these constraints.

LAWMAKING AND LEGISLATURES

Hurst begins his treatment of the growth of American law with the legislature, followed by the courts, the bar, and the executive. Democratic governments normally require their legislatures to approve the statutes that in turn, authenticate most governmental policies and decisions. What varies is the principal locus of, and participants in, statute making. In parliamentary systems that work is often done at the behest of a supreme committee of the legislature, the cabinet, with the main body performing debating, modifying, and ratifying functions. In one-party, presidential systems, statute making may occur mostly in the executive, with the legislature ratifying the result.

In a separated system like that of the United States, statute making is exquisitely complex, because by constitutional directive, the executive and legislative branches have active roles to play and by judicial reading, the Supreme Court has the authority, on the call of a case, to test what has been decided against the understanding justices have of the basic charter. As a parochial Americanist, I begin where my interest and training have led me—with a discussion of statute making in the separated system. It is my intent, however, to do so in a manner that encourages and strengthens comparative, historical, and cross-institutional analysis and that places statute making in the broader context of lawmaking as defined here. I am, in short, attempting myself to break free from the constraints of an institutional focus. But a strong case can be made for doing so by making one's sightings from familiar territory—in my case, statute making. The advice, perhaps, is to start with what you know but to look outward, not inward.

I begin by asking, What are the principal features of a legislature in a democratic system? It is the variation among these characteristics that should help to explain differences in how and what statutes are enacted among political systems.

I observe that democratic legislatures are representational, institutional, and partisan. The *representational* feature confers legitimacy by linking legislators with the public, typically by elections. *Institutionalism* is the means by which a legislature seeks stability by carrying forward those rules, procedures, and structures that advance its performance. *Partisanship* is a facilitative grouping of legislators, typically in terms of ideology, a core of policy views, or perhaps historical relationships. Legislators find it convenient, often necessary, to seek election as partisans, then to organize as such within the legislature. Among its other advantages, partisan organization provides leadership, typically on the basis of divergent policy views.

Clearly, political systems vary in significant ways in regard to these three contextual features. These differences, however, hardly mask an important common attribute that invites comparative inquiry. Democratic legislatures at work are showplaces for what has been and what is coming next. Rules, order, and organization provide continuity, the place to start in making law. Representation constantly challenges the existing order, sometimes overthrowing it. Partisanship contributes the means for effecting peaceful change by offering an indulgent setting for those who think alike on major issues of the day.

STATUTE MAKING

Political scientists have produced a substantial literature on the three contextual features of legislatures, if not always on their interactive effects or their full bearing on statute making. Attention here is directed less to legislatures as political bodies than to statute making. I am not, however, simply promoting stories of how a bill becomes a law. Rather, I will identify attributes of statute making that, in my view, invite comparative study, establish a substantive basis for the study of process, and therefore have the potential for theory building. To reiterate, this exercise is my effort to capitalize on and expand that which is familiar to me so as to promote the broader understanding of laws and lawmaking.

Statute making in democratic systems is continuous, iterative, informative, deliberative, speculative, sequential, orderly, and declarative. To explain briefly, *continuity* refers to the fact that most new law in a mature government is derivative of existing statutes and their implementation; *iteration* directs attention to successive alterations as proposed legislation moves through the lawmaking process; *informativeness* indicates the fact-finding and educational functions of statute making; *deliberation* identifies the forms of discussion and debate that are characteristic of the process; *speculation* alludes to the estimates of effects that are characteristic of lawmaking as social experimentation; *sequence* refers to the stages in the progression of statute making and surely varies substantially among democratic systems; *orderliness* concerns the setting of priorities or rankings among proposed statutes; and *declaration* pertains to the publication of agreements through the course of statute making.

As expected, these features of statute making in a legislatures will be shaped by the contextual variables for that institution. For example, representation and partisanship in their various forms contribute to iteration—indeed, guarantee it. Institutionalism reinforces continuity and designates sequence, as well as granting the means for securing order and declaring agreements. All three influence *what* information will be developed and communicated, and *how* it will be. The amount and type of information aid us in understanding statute making as a speculative enterprise. Further—and also as expected—the attributes of statute making have interlocking effects that beg for fuller explication.

Four of these eight attributes—continuity, deliberation, sequence, and orderliness—are, I believe, commonly understood and have been the subject of sufficient scholarly treatment by legislative scholars to invite comparisons among political systems.[4] The other four features—iteration, inquiry (information gathering), speculation, and declaration—are less well understood and yet appear to satisfy needs identified here, namely, a strong potential for cross-institutional and cross-national analysis of lawmaking and the means for studying the substance of lawmaking without resorting to story telling about how a bill becomes a law and how a law becomes a program.

Iteration

Statute making is iterative. It may be viewed as a progression of alterations. A policy idea is tested by those interests with a legitimate right through representation to introduce and promote change on their own. Democratic political systems naturally vary in the number and locus of public access to decision making, as well as in the scope of allowable alteration at various stages. Consider statute making in the U.S. Congress. The bicameral feature was designed in part to provide assorted perspectives on the public business. Given their coequal status, one could not imagine that either the House of Representatives or the Senate, save for a strategic purpose, would forgo their prerogative to participate actively in lawmaking. In fact, the members in each chamber tend to guard their independence, reacting critically to policy strategies that are perceived as threatening that status.

In the separated system, coequality of the two chambers and of the Congress itself with the other two institutions contributes to the need for a fully articulated structure that facilitates iteration. That structure includes the most refined and active committees of any legislature in the world—formidable policy units replete with user-friendly memories, a modified seniority-based structure, and a leadership-oriented staff. There are virtually no issues on the national policy agenda that have not been visited by congressional committees. (At least, there are no issues that lack file entries or alternative proposals.)

In a separated system like the United States, much can be learned about iteration in statute making by concentrating on legislatures. The same cannot be said for parliamentary and presidential systems, where the role of the legislature is more circumscribed by partisan or coalition governments or executive power. Iteration is more likely to occur within a ministry, a cabinet, the bureaucracy, or party councils. These differences in who participates in the iterative process and the scope of permissible alterations, as well as whether, when, and how closure is achieved, suggest a number of interesting questions in comparing political systems by this attribute. For example, are there significant substantive differences in statutes on the same issue when iteration is predominantly bureaucratic as compared to when the legislature is actively involved? Are the former technically sound and politically risky? Are the latter just the opposite? Which is more easily administered or more likely to gain public support? What is the nature of post-statute-making iteration among various democratic systems? How does discretion vary when the law is revisited at later stages? These questions suggest a distinction between open-ended and managed (or controlled) iterative processes that is potentially useful in comparing political systems, as well as in assessing the treatment of public issues in any one system.

Inquiry

Statute making requires information. The plain characteristics of writing and enacting laws intimate the strategic relevance of inquiry. To act on public issues requires a knowledge of their nature and of proposed solutions. Elijah Jordan expressed it well: "The *necessity* of making law becomes the *obligation* to know. And the obligation to know has for its objective the willed order of fact which is life" (1952, 153). Representatives must find out who to represent for what purpose. Even Edmund Burke's "trustee" had to offer wisdom that presumably was not wholly inherited. Various rationales are relied on for locating the fact-finding function of lawmaking in one or another of the institutions. For some, it belongs in the legislature because of its electorally based legitimacy and representational function. For others, the need is for specialists, for a cadre of experts whose fidelity is to science, not constituents. For the United States, Hurst observed that fact-finding authority is naturally associated with legislative work: "Law represents an effort—however short of the ideal—to order men's affairs according to rational weighing of values and the means of achieving them; how the lawmaker learns the facts of the living society in which he intervenes is, therefore, a point of fundamental importance regarding the manner of lawmaking. No agency in [the U.S.] government inherited a fact-gathering authority in any degree comparable to that of the legislature" (1950, 25). Tracing the "broad power to investigate" to the House of Commons in the sixteenth and seventeenth centuries, Hurst explained that this "authority continued to be recognized in the almost unchallenged practice of the Continental Congress, the Congress under the Federal Constitution, and the legislatures of the states" (ibid.). He pointed out that the legislature's fact-finding authority in the United States extends, as well, to determining information gathering for the other branches:

> This authority plainly included the power to look for the facts that the lawmakers believed relevant to their decisions as to what laws ought to be passed, as well as facts about the way in which the executive and judicial branches were working under laws that already existed. Holding the purse, the legislature had the means with which to start and support broad inquiries. This fact had another aspect: apart from traditional limits upon their fact-finding authority, other government agencies could in any case pursue only such inquiries as funds granted by the legislature might permit.
>
> These were the factors that gave the legislature the opportunity to become the principal lawmaker in the United States. (p. 26)

This authority to inquire into public issues is the measure of legislative power in a separated system. To argue that there are limits to fact finding is to compromise the independence of Congress. No president, no department or agency head, no military commander, no technician or scientist can legitimately claim a policy subject to be out of bounds to members of Congress, though the members can, and do, restrict themselves on occasion (primarily in regard to national security issues).

And what exactly are the facts? What is the information base upon which statutes are promulgated? Who controls the information needed by others? What level of knowledge is acceptable for taking action, and how does that vary among issues? How systematically is information gathered, processed, and evaluated? What expectations are generated by what is learned—expectations regarding alternative proposals or as to what will be accomplished by a statute? To what extent do these expectations become commitments that are the basis for evaluating policy and for enacting reforms?

The information imperative for lawmaking is attracting attention among scholars. Keith Krehbiel offers an information-based perspective on legislative organization. In explicating a legislative signaling game, he identifies the motivations for and structure of statute making, as well as establishing the need for information given the uncertainty "about the relationship between policies and their outcomes" (1991, 249). Krehbiel concludes that "committee power in Congress is informational. . . . More specifically, committees earn the compliance of their parent chamber by convincing the chamber that what the committee wants is in the chamber's interest. In this respect, committee power is not only inherently informational but also inherently majoritarian" (pp. 255–56). Krehbiel invites us to define substance into power, proposing that it is there but for the sighting.

Robert A. Katzmann (1989) explains that signaling is not limited to the committees and the chamber. The drafting of laws sends signals to agencies and the courts. The more open-ended iterative system, like that in the United States, may produce highly ambiguous signals, sending administrators and judges scurrying for clarifying information to aid them in interpreting intent and meaning. Those more controlled iterative systems (e.g., Great Britain's) "closely approach the textbook ideal of lawmaking" (p. 304).

Thus, as Katzmann intimates, the need for information in the making of statutes is not limited to separated, openly iterative systems. Of special interest is the matter of who does the fact finding in other systems. What do legislators know, and what is the source of their information? To what extent, and by whom, is the information collected by bureaucrats challenged by elected officials in parliamentary or presidential systems? Does information shape expectations and commitments in the same way in all systems? Or may we expect reduced uncertainty with the lesser role played by legislators? Exploring the informational basis for defining life through law should be irresistible to political scientists.

Speculation

Statute making is speculative. This is the feature that makes the study of lawmaking so exhilarating. Consider what is happening. Policymakers—some elected, some appointed, others hired, many self-interested, still others volunteering—participate in an authoritative process that presumes to reshape social and economic life. Farmers lose a crop to floods, sick persons need health care, industries foul the air and water, workers lose jobs, inflation increases, people hurt each other—and policymakers enact and implement laws as correctives. Election campaigns are often elaborate issue searches, with candidates proposing solutions: "Here is my plan for X." Will it work? The optimistic answer is *perhaps*. It is vitally important for a social system that certain persons are willing to venture in this way, given the risks involved. Lawmakers assume obligations and bear responsibilities that others often shun. T. V. Smith put it, "Legislatures exist to solve . . . otherwise insoluble problems" (1940, 29).

No one, to my knowledge, has written more eloquently on statute making as speculation than Elijah Jordan in his intriguing 1930 "essay on the dynamics of the public mind," entitled *Theory of Legislation*: "Legislation as the process of the application of will and intelligence to the practical problems of life is in its first motive an act of the speculative imagination" (1952, 313). The process itself depends heavily on "talk." It is therefore reasonable to apply a kind of "talk test" to the speculative feature of statute making.[5] "Discussion is the method by which ideas get cleared up as to their logical consistency with each other, and the formal statement of ideas in judgments under the pressure of critical comparison with other ideas constitutes that experimentation under logical conditions which we have accepted as the definition of legislative thinking. A parliamentary body is a laboratory whose function is to provide these conditions" (p. 346). Jordan explained that "the legislator also devises instruments [administrative and judicial bodies] through which ideas of policy are realized" (p. 133). In the doing, lawmakers reach beyond themselves: "*Man's vision outreaches his power of action*, his eye sees where his hand cannot reach" (p. 144, emphasis original). The risks are substantial. As Senator Daniel Patrick Moynihan (D–New York) explained in reference to health care reform, "You get into pretending to know the unknowable" (quoted in Priest 1994, A4).

The speculative nature of statute making can be used as a basis for rationalizing a separated, checked, and balanced representative government. Who among us has absolute confidence that one person's or one group's "vision" will be acceptable? "Whoever insists that he can state other people's position as well as they can is a dictator at heart; he has the psychological equipment if only he can add to it the necessary power" (Smith, 1940, 13). One corrective, apart from the high access of an openly iterative process, may be accountability. If the one "vision" turns out to be wrong, then we know precisely who to blame.

Yet the costs of pure accountability for speculative failures may be very high: social and economic disruptions, human tragedy, war. Therefore the reality of speculation as characteristic of statute making and the anxiety regarding its results justify inviting many visions for resolving public problems. And so the speculative nature of statute making induces competition and compromise among legitimate participants, fashioning the enterprise as one of successive approximations. The consequences—disjointed incrementalism as a hedge against speculative failures and diffused responsibility among the elected and career officials with the right and responsibility to speculate—are often distasteful to those preferring classic responsible-party government. Thus speculation, as one of the basic features of statute making, is a stimulus for reform toward a certainty in decision making that is uncharacteristic of democracy and about which reformers themselves can only conjecture.

My present purpose is less to cope with the effects of competitive versus consolidated speculation than to encourage comparative study of this feature—across political systems, between institutions in the same political system, across issues, over time. A number of important questions require treatment: What is the basis in knowledge for speculation, and how does it vary among participants? What is the role of story telling or analogizing versus the more systematic or scientific methods of speculation? What are the expectations of participants for their plan or "vision"? Are these expectations modified through the iterative process of statute making? If so, how? Do they carry through to administration and evaluation of statutes? What conditions facilitate concentrated versus dispersed speculation? What explains a convergence, versus a divergence, of speculation among participants through the statute-making process? Comparative research would produce major insights on this central trait of statute making. We know how to find answers to these questions. And we should find a speculative enterprise of this order to be intriguing—after all, we are ourselves theorizers.

It is apparent that iteration, inquiry, and speculation are mutually reinforcing concepts in democratic systems. In fact, each can be subsumed by the other. Speculation is an act of intelligence, one obviously requiring information. Participants have personal and organizational sources of information; many rely on analogies, precedents, and experience. Interacting with others, they become aware of the informational basis for contrasting and complementary speculation. They may, as a result, be persuaded to alter their plan or to acknowledge the force of an alternative with others whose support is needed. Thus does competitive or segmented and differentially informed speculation lead to iteration in statute making.

Declaration

Statute making is declarative. Still another reinforcing concept in this interactive process of statute making is that of declaration. Here is yet another common feature to which we have paid little heed. Jordan points out that "the type of problem involved in practical life necessitates that the speculative process be also a public process, since life posits . . . a wider area for action than the will of the individual can comprehend" (p. 314). I argue that it is not only the type of problems dealt with in legislation that demands a public process but, as well, the need to keep track of what is happening where and when. Statute making in a democratic system requires declarations of the visions, plans, and agreements and various versions of the facts, so that others may test what is proposed by their own rendition of reality, however it is that their story has transpired.

Often these declarations are made part of the rules in the U.S. Congress, with (for example) reports, calendars of business, time requirements prior to floor consideration, official objectors, and "holds" all playing a role. Political parties and interest groups, too, are important for advertising agreements to their members, with the media informing the public, as well as stimulating further communication among policymakers. If anything, declarations are more vital than ever with the magnitude of statutes dealing with issues like trade, tax, regulatory, welfare, and health-care reforms. One measure of the growing significance of declarations is the reaction to efforts to plan in seclusion, even at the very early stages of policymaking. Presidents Carter, Reagan, and Clinton were criticized for preparing energy, economic, and health-care proposals, respectively, behind closed doors. The rationale in each case was to escape the pressure of "vested interests." Yet these interests believed that they should have the right to know what was being planned and to offer their own policy speculation in an official setting.

Statute making, then, can be properly understood as a series of *agree-and-display* exercises, with each iteration declared to a wider and wider audience for their response. Typically accompanying the agreement are revelations as to how it was reached—including the compromises and the breadth and depth of support. With proposals as big as comprehensive tax or national health-care reform, the declaratory process becomes an intriguing subject of study in and of itself. When to display what to whom is a strategic consideration of some moment, particularly given the number of decision makers and the broad effect of decisions made.

As with the other features of statute making, declaration entices comparative study. What is the scope of declaration or display of agreements in other democratic systems? To what extent is declaration managed by the majority party or coalition? What is the role of the strategic leak? Does declaration extend to the administration of statutes, as, for example, to rule making? Are there discernable effects of a more open and sequential declaratory process on public support for and acceptance of statutes?

LAWMAKING BEYOND STATUTES

Statutes are the beginnings of life through law. They represent an agreement at one point in time regarding a public issue. Thus it is that those few works on lawmaking (e.g., Hurst, Jordan) are comprehensive treatments on the work of government. Conceptually, statutes become the law only through interpretation and application. Here then is a means by which political scientists can be more integrative and less institution- and country-bound. For example, to study the politics of environmental law is to carry the research beyond statute making to determine how it is that the words of a statute come to have meaning in their application and review. Clearly, as scholars, we cannot remain fixed on one institution if we are to understand lawmaking. Even if we limit study to statute making, no such isolation is possible—especially if we intend to make comparisons on that basis.

Would we be merely duplicating the work of legal scholars? I think not. We bring a unique perspective to the study of lawmaking—that of politics. We are interested in more than the words that form statutes and how those words are read. We are also interested in the bargaining, the fact finding, the speculation, the testing by declaration, the manifold actions and interpretations that transform words into actions. And our purpose is not, typically, as participants in the making of new laws or as users to serve and interpret existing laws. Rather, we seek to understand the workings of government through study of one of its crucial functions.

I reiterate that I am not inviting abstract musings, however dense may be my prose. Speak with the lawmakers, whatever their institutional locale. They know the law for what it is—a set of past agreements interpreted and applied adaptively over time. They are sensitive to how changes will be received. Those estimates are a significant part of the speculative enterprise of statute making and revision. As political scientists, we can reconstruct how this all works for different governments over time.

TESTS OF LAWMAKING CAPACITY

Comparative scholars give the American system (typically labeled "presidential") relatively low marks when measuring its capabilities against those of parliamentary systems. As R. Kent Weaver and Bert A. Rockman put it, "Governing institutions, especially in the United States, have been strongly criticized for lacking in effectiveness" (1993, 7). Many Americanists, too, are critical and some are moved to propose constitutional reform (see Sundquist 1992). And media analysts often comment disparagingly about the workability of the separated system.

A diffused-responsibility government of mixed representation, disconnected elections, and multiple access points does not show well. Therefore one may expect, even hope for, continued criticism. There is little prospect of major constitutional change in the system, as is conceded even by the most ardent reformers. Political scientists, therefore, have an incentive to contribute dispassionate analyses of governmental capacity. An effective means for doing so is to study lawmaking. I also estimate that now is the time to do so. Here is the argument.

The United States is in a policy phase characterized by large-scale proposals that test the capacity of its political system. Many of the New Deal and Great Society programs are at a point where reevaluation and reform are inevitable. Previously unimagined deficits have stimulated a review of what works and what doesn't. The persistence of certain issues on the agenda despite sizable government programs has stimulated programmatic reform. Dramatic changes in international politics have had numerous effects on domestic politics. Other great nations, too, have undergone virtual transformations that have tested the capacity of governmental institutions to cope.

It is therefore a propitious time to study lawmaking. Large-scale proposals like those reforming the tax code, welfare, health-care delivery, the criminal code, trade, the regulatory system, and education challenge the representational, institutional, and partisan features of Congress. Lawmakers are sometimes engulfed by the communicators, adherents, and adversaries of change. The normal tension between representing change and preserving stability is amplified considerably. Then there are the effects of large reform packages on the four attributes identified here. Iteration is a multiphased process with a large number of participants. Information gathering is continuous and often redundant due to the independence of participating units. Speculation may vary through the process of statute making—unguarded in the early stages as reform is initiated, circumspect in the later stages when proposals are modified and inquiry reveals the risks involved. Declaration becomes highly strategic as lawmakers consider how much to reveal, to whom, and when. As statute making proceeds, declaration is less and less in the control of one person or group, further complicating decision making for any one set of participants. Health-care reform in the United States is a prime contemporary example of each of these developments.

The study of lawmaking will produce the basis for comparative judgments about governmental capacity and effectiveness. We have hardly begun that task, in my view. It is it time we started, because the challenges of large-scale reform and change provide an opportunity to identify the limits of the system. As Bismarck intimated, watching laws being made is not a pretty sight. My personal view is that those in search of beauty should visit an art gallery or canoe down the Wisconsin River on an October afternoon. One of our tasks as scholars is to understand politics as it is so that others may get it right when they are moved to make change.

The theme of the nintieth annual meeting of the American Political Science Association is "Politics and Political Science in a Changing World." Great political systems are wrestling with the types of large issues that prompted the theme: Russia and China with economic reforms, Japan with electoral reforms, Great Britain with its place in the European Community, the United States with health care and welfare reform. Other nations are coping with even more fundamental issues of constitutional structure and institutional arrangements. Lawmaking is a pivotal decision-making process by which these matters will be acted on. Statutes will be the basis for the laws that affect social and economic life. Analysis of the iterative, informational, speculative, and declarative activities will inform us how it is that these various political systems do their work, thus providing a basis for comparison that is grounded in the substance of politics, as well as a means for judging capacities to govern.

Thomas Brackett Reed, Speaker of the House in 1889–91 and 1895–99, proposed his own version of the ways of life and law. For him, "freedom never meant the best government in the abstract, it only meant the government best fitted to the people governed. We have not the best laws in the United States that wise men could dream of. What we have [are] the best laws our people are fit for; and as they grow in knowledge and sense the laws follow in laggard procession. But they follow" (McCall 1914, 256).

It is this fascinating process by which the laws follow the growth in knowledge that should stimulate our research impulses. Ultimately, any government will be tested by its capacity to make laws of a quality to meet the needs of its citizens. To the extent that we as political scientists are prepared to explain and predict the procession of laws, we justify our professional craft and create a need for what we have learned.

Notes

I acknowledge with gratitude the comments on an earlier draft by Thomas E. Mann, Robert A. Katzmann, and J. Willard Hurst.

1. In their comprehensive bibliography on the U.S. Congress (5,630 entries), Robert U. Goehlert and John R. Sayre (1982) have no entry for "lawmaking" in either table of contents or subject index. There are sections for "Legislative Case Studies" (one of the smallest sections) and "Legislative Analysis" (with a majority of the entries being voting studies). The *Handbook of Legislative Research* (Loewenberg, Patterson, and Jewell 1985) includes 16 chapters, none of which is on lawmaking as such. Many of the chapters are, to be sure, relevant to an understanding of lawmaking. But the volume indicates that scholars are more interested in elections, voting, committees, and leadership than in lawmaking.

2. Here is the count of articles for three journals—*American Political Science Review, American Journal of Political Science*, and *Journal of Politics*—for the years 1983–93: high return of incumbents and related topics, 36 articles; party voting in presidential elections and related topics, 35 articles; and health policy and politics, 4 articles.

3. Distinctions are often made within these broad categories, however. For examples, see Weaver and Rockman 1993, chap. 1, and Shugart and Carey 1992, chaps. 5–8. See also Lijphart 1992 for a sample of literature on presidentialism and parliamentarism, including the criticisms often leveled at the former.

4. For example, in the United States, continuity is evident from much of the literature on the policy process, with the other attributes—deliberation, sequence, and orderliness—treated extensively in studies of committees, floor debate, and the rules. Of the four, sequence has been subject of the least study, despite the urging of Richard F. Fenno, Jr. (1986) in his presidential address, "Observation, Context, and Sequence in the Study of Politics."

5. Arthur Maass emphasizes the importance of policy conversation and deliberation in a "discussion model" (1983, chap. 1).

References

Fenno, Richard F., Jr. 1986. "Observation, Context, and Sequence in the Study of Politics." *American Political Science Review* 80:3–16.

Goehlert, Robert U., and John R. Sayre. 1982. *The United States Congress: A Bibliography*. New York: Free Press.

Hurst, J. Willard. 1950. *The Growth of American Law: The Lawmakers*. Boston: Little, Brown.

Jordan, Elijah. 1952. *Theory of Legislation: An Essay on the Dynamics of the Public Mind*. Chicago: University of Chicago Press.

Katzmann, Robert A. 1989. "The American Legislative Process as a Signal." *Journal of Public Policy* 9:287–305.

Krehbiel, Keith. 1991. *Information and Legislative Organization*. Ann Arbor: University of Michigan Press.

Lijphart, Arend, ed. 1992. *Parliamentary Versus Presidential Government*. New York: Oxford University Press.

Loewenberg, Gerhard, Samuel C. Patterson and Malcolm E. Jewell, eds. 1985. *Handbook of Legislative Research*. Cambridge: Harvard University Press.

Maass, Arthur. 1983. *Congress and the Common Good*. New York: Basic Books.

McCall, Samuel W. 1914. *Thomas B. Reed*. Boston: Houghton Mifflin.

Priest, Dana. 1994. "Where Health Care Reform Effort Failed." *Washington Post*, 27 August.

Shapiro, Martin. 1989. *Studies on American Political Development*. New Haven: Yale University Press.

Shugart, Matthew S., and John M. Carey. 1992. *Presidents and Assemblies: Constitutional Design and Electoral Dynamics*. New York: Cambridge University Press.

Smith, Thomas V. 1940. *The Legislative Way of Life*. Chicago: University of Chicago Press.

Sundquist, James L. 1992. *Constitutional Reform and Effective Government*. Rev. ed. Washington. Brookings Institution.

Verney, Douglas V. 1992. "Parliamentary Government and Presidential Government." In *Parliamentary Versus Presidential Government*, ed. Arend Lijphart. New York: Oxford University Press.

Weaver, R. Kent, and Bert A. Rockman, eds. 1993. *Do Institutions Matter? Government Capabilities in the United States and Abroad*. Washington: Brookings Institution.

Charles O. Jones is Glenn B. and Cleone Orr Hawkins Professor of Political Science, University of Wisconsin-Madison, Madison, WI 53706.

American Political Science Review Vol. 89, No. 1 March 1995

THE CONCEPT OF A CRITICAL REALIGNMENT, ELECTORAL BEHAVIOR, AND POLITICAL CHANGE

PETER F. NARDULLI *University of Illinois, Urbana–Champaign*

The realignment perspective has exerted an enormous amount of influence on thinking about American politics, but recently it has fallen into disfavor. As a theory of political change, this dissatisfaction is warranted. However, in rejecting the realignment perspective, scholars risk losing a valuable concept, the notion of a critical realignment. My thesis is that, properly defined, the concept of a critical realignment can be a powerful tool in the study of electoral behavior and an important component of a broader theory of political change. This thesis derives from an analysis of presidential elections between 1828 and 1984. This analysis provides dramatic evidence for the proposition that critical realignments are important electoral phenomena. The evidence is equally clear, however, that critical realignments are subnational phenomena that vary considerably in form, not the majestic national movements some believed them to be. The analyses reported here reveal broadly based electoral eruptions of 40 to 50 points that endure for decades.

In his seminal article on critical elections, V. O. Key (1955) attempted to formulate a partial theory of elections. Intrigued by the broad implications of his formulation, scholars quickly transformed it into a "democratic" theory of political change—the realignment perspective on American politics (Clubb, Flanigan, and Zingale 1990; Kleppner 1987). This perspective has exerted an enormous amount of influence on thinking about American politics, for two reasons. First, it offers important caveats to the "pluralist-incrementalist" paradigm (Almond 1988) by offering hope for nonincremental change in a government structured in the late eighteenth century to block the whims of transient majorities. While its complex system of checks and balances prevents the U.S. government from acting hastily during normal times, critical elections—the realignment perspective asserts—mobilize and integrate cumbersome institutional networks during political crises (Brady 1988; Burnham 1970). Second, this perspective resurrects voters from the shadows to which they have been relegated since the beginning of the behavioral tradition. Voters, not elites, are the dynamic component within the realignment perspective. Responding to crises and broad socioeconomic developments, they generate the political force that precipitates the mobilization and integration of policymaking institutions.

Despite the attractiveness of the realignment perspective and its importance to American democratic thought, it recently has fallen into disfavor. Students of critical realignments can agree on little more than that the empirical findings in this area have yielded less than clear-cut results. While such ambiguity is not uncommon in social science research, it is disturbing in an area concerned with seismic changes in electoral behavior. If these seismic changes are to have ramifications throughout the political system, then social scientists analyzing electoral patterns over time should be able to detect them with a high degree of precision and agreement. Carmines and Stimson capture nicely the frustrations of scholars unable to do so:

> If simplicity is often the first casualty of scientific research, that is even more the case in realignment research: the simple and dramatic model of critical realignment, the one that so readily captures our attention, finds no supporting evidence. . . . The closer we look, the more these "simple" realignments become movements over time, taking decades or multiples of decades to achieve their final form. When precursors and aftershocks are added, the multiple decade processes overlap, and the continuing effects of old movements are still manifested while new and different movements toward a still new alignment are underway. There is no "normal" period left, no time when the reshaping of loyalties and behaviors is absent. . . . [Thus] the realignment literature may be seen as a long string of amendments by typology, as successive scholars have applied the concepts to the raw materials of (usually American) political history and found the empirical materials too complex to be subsumed by the initial concept. (1989, 21–23)

Most objective reviewers of the realignment literature would not disagree too strongly with this characterization, which forms the basis for Carmines and Stimson's classification of the concept of a critical realignment as "prescientific." Defying empirical refutation, they argue that the notion of a critical realignment is too resilient to be useful as a scientific concept. Empirical inconsistencies merely become the basis for provisos and refinements, impairing the concept's utility in guiding empirical research.

Carmines and Stimson are correct in underscoring deficiencies in the prevailing views of a critical realignment. But these deficiencies do not necessarily mean that this concept has no theoretical value. Because the concept of a critical realignment is so beguilingly simple, and its potential ramifications so important, realignment scholars moved quickly—too quickly—to the construction of a broad, encompassing theory of political change, the realignment perspective. This developmental path put critical realignments at the center of the study of American political change but also led to a neglect of the concept itself. It has never received the scholarly scrutiny it deserved, nothing that would compare, for example, with the scrutiny given to the concept of party identification. This is unfortunate because the concept of a critical realignment, like that of party identification, is not as straightforward as it appears—as we shall see shortly.

The failure to develop and refine the concept of a critical realignment produced a weak foundation for a theory of political change, causing a great deal of confusion, dissatisfaction, and frustration. However, rather than leading scholars to reexamine the theory's conceptual base, this consternation led to pleas to abandon the idea of a critical realignment altogether, or to limit it severely (Shafer 1991). These pleas, I intend to demonstrate, are ill founded and would lead to the loss of a valuable *concept*, the concept of a critical realignment. My thesis here is that if properly defined, the concept of a critical realignment is a powerful tool in the study of electoral behavior. More importantly, it can be an important component of a broader theory of political change.

To develop this thesis, I first offer a restatement of the concept of a critical realignment and briefly describe a normal vote approach to detecting critical realignments. Next, I examine this reconceptualization by applying the normal vote approach to presidential electoral patterns. The data generated by this analysis reveal if, when, and where critical realignments in presidential voting patterns occurred in the United States since 1828. They also provide measures of the impact of these realignments on prevailing electoral patterns. I close by developing the implications of this restatement for the study of political change.

CRITICAL REALIGNMENTS: CONCEPTUALIZATION AND METHODOLOGICAL APPROACH

The notion of a critical realignment is an aggregate-level concept that refers to an abrupt, large, and enduring form of change in prevailing electoral patterns, one that is initiated by a critical election and results in a significantly different partisan balance in the electorate. This much is clear from the literature on critical realignments. What is less clear is that in order to understand the role of critical realignments in structuring electoral patterns, scholars must be sensitive to considerations of both time and space. In order to detect and gauge enduring changes in electoral patterns accurately, scholars must examine long time frames and be sensitive to the fact that enduring critical change can assume a variety of forms. Equally important for the detection and measurement of realignments are spatial considerations. It is unrealistic to expect the entire U.S. electorate to respond simultaneously and uniformly to the type of stimuli that will generate a critical realignment in electoral patterns. There is simply too much geopolitical diversity in the United States to justify such an expectation.[1]

These observations lead us to expect considerable diversity in the impact of critical realignments on prevailing electoral patterns over time and space. These expectations can be examined by analyzing results from a study of presidential electoral patterns based on data from every county and most major cities (3,136 counties and cities and 107,974 unit-elections) in the continental United States for every presidential election between 1828 and 1984. The units of analysis in this study are 215 substate regions. These regions are geographically contiguous counties that share similar electoral patterns over time, or they are major cities that manifest distinctive electoral patterns within a state. I report the procedures used to construct these regions elsewhere (Nardulli 1994) and summarize them in the Appendix.

The analyses of electoral patterns I shall report are based on normal vote analyses of electoral change in each of these 215 regions. Thus, before the results of these regional analyses can be presented, a brief overview of the normal vote approach to electoral change will be useful. I provide more details elsewhere (Nardulli 1994) and in the Appendix.

The Normal Vote Approach to Electoral Change

The normal vote approach I employ here uses interrupted time-series analysis to map the flow of electoral change over time across subnational regions. This technique requires the analyst to specify the point at which major, enduring interruptions in long-term electoral trends (critical elections) are hypothesized to begin, as well as the form of those interruptions. It also tests whether each year hypothesized as a critical election in fact marks the beginning of a statistically significant interruption in electoral patterns. The forecast from a time-series analysis reflects the deterministic component of a time series, which is derived by filtering short-term disturbances (noise) from the raw data. In the case of the electoral time series being analyzed here, these forecasts can be interpreted as normal vote estimates that are useful in depicting prevailing electoral patterns.

These normal vote estimates also can be used, in conjunction with the data on critical elections, to gauge the impact of a critical realignment on prevailing electoral patterns. Although calculating the overall electoral impact of a critical realignment is quite involved (and will be addressed later), measuring its *initial impact* is rather straightforward and nicely illustrates the utility of the normal vote approach in the study of critical realignments. A realignment's initial critical movement is measured by subtracting the value of the normal vote estimate in the year preceding a critical election from its value in the critical election year.[2] I used a margin-of-victory variable (Democratic % of total vote − Republican % of total vote) to conduct the interrupted time-series analyses. Thus the normal vote estimates are expressed in terms of the margin of victory normally enjoyed by a party in a region. A value of .10 would indicate a 10-percentage-point cushion normally enjoyed by the Democrats, while a value of −.10 would suggest the same for the Republicans. If the value of the normal vote estimate for a region was −.10 in 1928 but jumped to .30 after a critical election in 1932, the magnitude of the initial impact would be .40, in favor of the Democrats. Correspondingly, a movement from −.05 to −.25 after the critical election of 1896 would suggest an initial impact of −.20, in favor of the Republicans.

One form of realigning change that will not be detected by this approach is a coalitional shift among important electoral subgroups that does not result in a significant change in the partisan balance of the regional electorates analyzed here. Consider, for example, a situation in which industrial workers shifted in large numbers to the Republicans while farmers shifted to the Democrats in equally large numbers, resulting in a very different coalitional structure within each party. *If the shift occurred at the same election and if the magnitude of the shift within each of the 215 electoral regions was roughly equal,* then this important change would not be picked up by the normal vote approach developed here. If, however, the geographic distribution of these groups were different and changes in regional partisan balances occurred, then the coalition shift would be captured by this approach—even if the overall shift at the national level is equivalent and no overall shift in the national partisan balance is generated. The change would be undetected only in regions where the coalitional shift produced no change in a region's partisan balance.

In sum, for realigning change that produces a shift in the partisan balance within regional electorates, this approach permits a precise specification of *whether* a critical realignment occurred as well as *when* it occurred. Because this approach was implemented at the subnational level, it also provides a precise specification of *where* the interruptions occurred. Finally, the normal vote approach permits us to gauge the *impact* of these interruptions on prevailing electoral patterns. We can now examine the results from the normal vote approach to illustrate each of these points.

THE INCIDENCE OF CRITICAL ELECTIONS OVER TIME

If, as some critics claim, the concept of a critical realignment lacks an empirical referent, the normal vote approach just described would yield largely null results. Null results would obtain if (1) there are no marked and enduring breaks in prevailing electoral patterns or (2) there are no prevailing electoral patterns (i.e., vote outcomes are determined largely by short-term forces resulting in highly volatile time series). As suggested earlier, however, these null results do not obtain. Most regions with vote returns for the entire 156-year period experienced three to four critical realignments; a few had as many as five, while some had only one.

The mere rejection of the null hypotheses in the 215 analyses conducted here is not very telling for present purposes. It is insufficient to argue that critical realignments are important political phenomena merely because they are prominent in regional electoral patterns. Unpatterned interruptions detected at the substate level, no matter how large, could be little more than random eruptions. To be truly consequential political phenomena, the regionally detected realignments uncovered here must have some temporal ordering when viewed in the aggregate. That is, the critical elections detected here should cluster around a handful of elections. If they did not, it would be difficult to argue that critical realignments reflected a widespread electoral response to important political stimuli.

We can determine whether this temporal ordering exists simply by examining the incidence of critical elections by year. If the realignments identified here are merely random eruptions, then no elections would stand out as prominent electoral watersheds. Figure 1 reports the weighted proportion (for the nation as a whole) of all regions that registered a critical election, by year and party.[3] It reveals, for example, that over 40% of the national electorate lived in regions registering a critical election in 1856. Most of these shifts favored the newly formed Republican party. In 1932, over half of the national electorate lived in regions that registered a critical election; all of these shifts favored the Democrats. No critical realignments at all were initiated in 1844, 1908, 1956, and so on.

Figure 1 provides strong evidence that the regional realignments identified here have a discernible temporal structure. Most of the critical elections that occur (88%) fall in or around six elections—1836, 1856, 1896, 1932, 1948, and 1960—or in the South between 1876 and 1904.

The realignment eras that emerge from this analysis of critical elections can be categorized readily. I label the first realignment the Whig realignment, as it roughly coincides with the emergence of the Whigs on the national scene.[4] A minor set of Democratic antebellum critical elections began in 1852 and continued through 1856.[5] The Republican antebellum realignment began in 1856. The period between 1864 and 1904 looks unstructured. But most of the critical elections during that era occurred in the South and are part of what I will refer to as the Jim Crow realignment.[6] The pro-Democratic critical elections of 1896 began the Democratic populist realignment, while the pro-Republican critical elections of 1896–1900 started the Republican industrial realignment. I will refer to the 1928-1932-1936 Democratic realignments as the New Deal realignment, the 1948-1952 Republican sequence as the post–World War II realignment, and the 1960 Democratic surge as the New

Frontier realignment. These labels will be useful in the graphic presentations that follow.

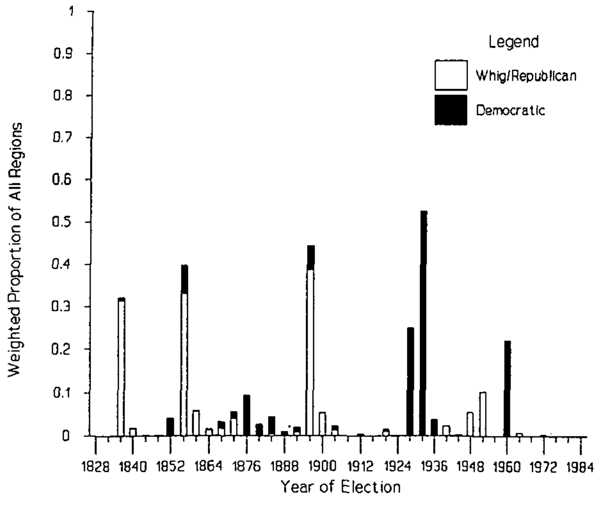
THE DISTRIBUTION OF CRITICAL REALIGNMENTS OVER SPACE

The data in Figure 1 strongly support the utility of a subnational approach to understanding critical realignments. There has never been a truly national realignment. Indeed, with the exception of 1932, no critical election involved as much as half the nation. Using national-level data to detect critical realignments would have led to misleading results about both the incidence and the electoral impact of critical realignments in U.S. electoral history. However, contending that critical realignments are subnational phenomena requires more than establishing that they are not national phenomena. Just as we expected some temporal order to the incidence of critical realignments, there should be some spatial order.

The existence of this spatial ordering can be assessed simply by examining maps of the regions that realigned during the various eras identified earlier. Figure 2 reports those regions and provides strong empirical evidence for the type of spatial ordering we expect. Consider the Whig realignment (map a). It shows that most of the South and much of the West realigned in the period between 1836 and 1840. The only part of the Northeast that realigned was western Pennsylvania. Map b reveals a similarly clear but very different pattern. The Republican antebellum realignment at the presidential level was heavily concentrated in New England, upstate New York, and the upper West. Wisconsin is not really an exception here because its time series begins in 1856; while it voted strongly Republican in 1856 it had no base from which to realign. Interestingly, there are a smattering of Missouri and Southern regions (northern Alabama, central North Carolina, and northwest Arkansas) that register a Republican realignment, but very few in the central part of the country (southern Indiana, Ohio, Pennsylvania, etc.).

FIGURE 1

Distribution of Critical Elections, 1828–1984



Because the Democratic antebellum realignment affected so few regions (see Figure 1), it is not displayed here. The more broadly based and enduring shift in Southern voting patterns generated by the events that precipitated the Civil War occurred during the Jim Crow era,[7] and its distribution is displayed in map c. Almost the entire South (east of Texas) experienced critical shifts during this era. The primary exceptions are some regions in the deep South that experienced a Democratic realignment in the antebellum period.

The Democratic populist realignment was both short-lived and narrowly based and it too is not displayed here. It was confined to three contiguous states: Kansas, Nebraska, and Colorado, plus northern Washington state. In contrast, the Republican industrial realignment (map d) was concentrated in the Northeast. There were a smattering of realignments in the South and the border states, as well as on the West Coast (northern California and the Seattle metropolitan area). This realignment looks very similar, in spatial terms, to the Republican antebellum realignment (map b).

The New Deal realignment (map e) is unmatched in its geographic scope. Most of the nation outside the deep South realigned. The exceptions here are most of Virginia and Ohio, upstate New York and southern Indiana, as well as parts of the plains states (most of the two Dakotas and Nebraska), and some regions in southern Missouri and Arkansas. Although the New Deal movement is in a class by itself, it cannot be interpreted properly without examining it in light of the Republican post–World War II realignment. If map f is compared with map e, it can be seen that one is almost the mirror image of the other. Most of the deep South realigned after World War II, along with some of the regions in Indiana, Ohio, Virginia, Missouri, Arkansas, and Nebraska that did not realign in the New Deal era.

I have not presented a map for the New Frontier realignment because, with some exceptions,[8] the regions that realign in 1960 are major urban areas: New York, Philadelphia, Boston, Chicago, Detroit, Cleveland, Indianapolis, Kansas City, Louisville, Baltimore, and Seattle. But while these cities are barely visible on a map of the United States, they constituted nearly 20% of the electorate in 1960, making this an important shift in American electoral history. Moreover, this realignment is important for reasons that go beyond its numerical scope. It is undoubtedly part of a political dynamic that began in 1928. The New Deal realignment broke the South's grip on the Democratic party, leading it to move in policy directions that led to the South's estrangement in 1948 and 1952 (see map f). The subsequent shoring up of Democratic strength in 1960 among urban constituencies is probably a consequence of the Kennedy candidacy and the party's reorientation that Carmines and Stimson (1989) document so nicely.

THE IMPACT OF CRITICAL REALIGNMENTS ON PREVAILING ELECTORAL PATTERNS

Figure 2 illustrates the diverse impact of political stimuli across different regions of the United States. We expect the same type of diverse impact over time. When viewed throughout U.S. history, it is clear that the critical realignments that have occurred have been generated by a different mix of political, moral, cultural, and economic forces. These different mixes cannot be expected to generate similar patterns of change over time. Nor can we expect the patterns generated within a realignment era to be uniform across space. These differences will be reflected in the magnitude of a realignment's initial critical movement, the course of the realignment after its initial movement, and its duration. These three factors combine to give a critical realignment's impact on electoral patterns a distinctive contour. The shape of these contours, along with differences in geographic scope (see Figure 2), means that some of these quintessentially democratic eruptions will register much higher on the electoral Richter scale than others. These differences must be considered when thinking about the political and policy consequences of the realignment eras identified earlier. To ignore the magnitude of the shock to the electoral status quo would be to overlook what must be a key factor in any attempt to understand the impact of popular sentiment upon political processes, especially for those interested in whether that impact has changed over time.

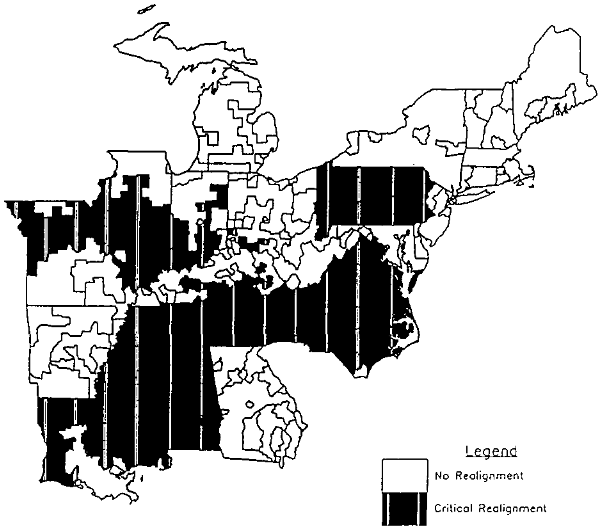
Measuring Electoral Impact

These considerations make it essential to capture the impact of critical realignments on prevailing electoral patterns. This requires a set of electoral impact measures and a multistage process to develop them. This process must begin with a *regional impact measure.* Such a measure must capture the cumulative electoral change generated by a realignment, beginning with the critical election that initiates it and ending with the point at which the realignment's force is spent (i.e., when it is interrupted by a subsequent realignment). Also important is an *aggregated regional impact measure*—a summary measure that aggregates the electoral impact of regional critical movements for an entire realignment era. Finally, a *national impact measure* is needed, one that captures the effect of a realignment era on national vote patterns. This measure must take into account the proportion of the national vote that was involved in the realigning regions.

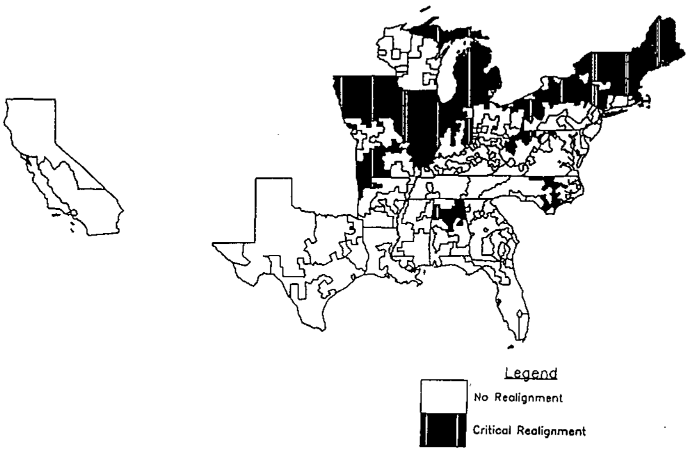
Capturing the cumulative changes from the electoral status quo requires that some attention be given

FIGURE 2

a. Whig Critical Realignments, 1836–40



b. Antebellum Republican Critical Realignments, 1856–60



c. Jim Crow Critical Realignments, 1876–1904

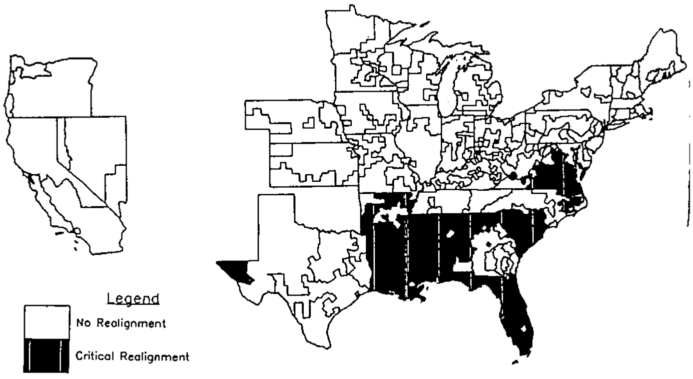
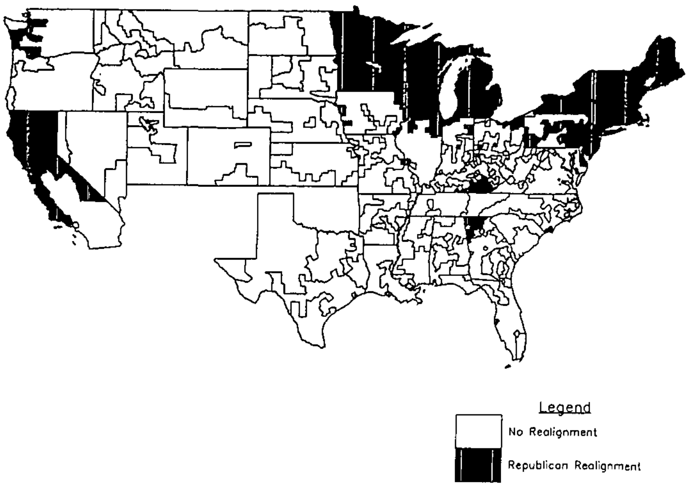
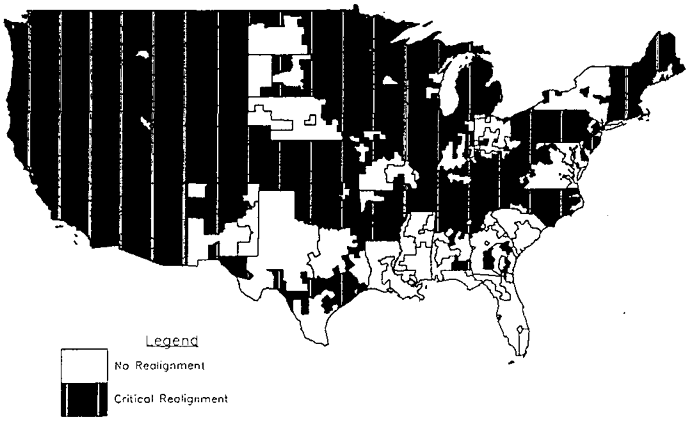


FIGURE 2 continued

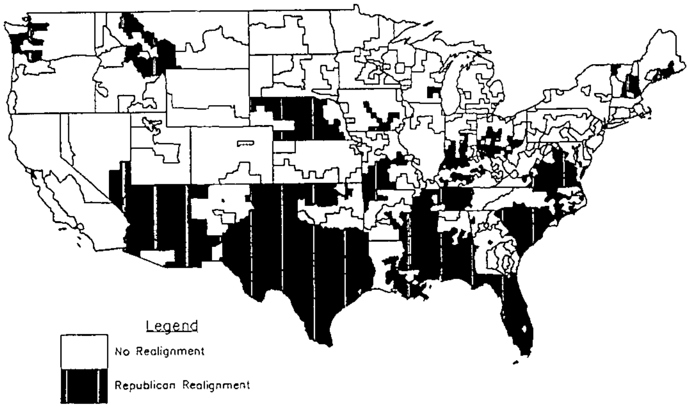
d. Republican Industrial Critical Realignments, 1896–1904



e. New Deal Democratic Critical Realignments, 1928–36



f. Post–World War II Republican Critical Realignments, 1948–52



to the various forms that critical realignments can assume. Although Carmines and Stimson (1989, 139–45) introduce four models of temporal change (the impulse-decay model, the step model, the dynamic growth model, the secular model) that emerge from the literature on interrupted time-series analysis (Box and Tiao 1975; McCleary and Hay 1980), they associate only the step model with a critical realignment. While this is consistent with most work in the field since Key, he had a much broader conception of a critical realignment.

The literature on time-series analysis was not developed when Key did his work, but he clearly understood that some decay of the initial critical movement occurred in at least some locales (Key 1955, 6–8). Indeed, this is almost a defining characteristic of the New Deal realignment, which served as his model. This decay was not troubling to Key because of the scale upon which these changes were taking place. An initial shift of 40 points that deteriorates by half over a twenty-year period is best fit by an impulse-decay model, in the parlance of interrupted time-series analysts. But to political observers such as Key, such a disruption in the electoral status quo is a major political happening! Had Key examined the Republican industrial realignment he would have noticed a different form of critical change, one that began with a 20-point shift but then grew over the course of several decades. A dynamic growth model would be required to capture such a change accurately.

Given these historical realities, it would be short-sighted to exclude the impulse-decay and the dynamic growth models as variants of critical electoral change.[9] Along with the step model, they fit comfortably within the definition of a critical realignment. Each model refers to an abrupt, enduring change in electoral patterns. The course of change after the initial critical movement is simply different for each.[10]

These differences are important to note because they have important implications for measuring regional electoral impact. Regional impact cannot be computed simply by combining the size of the initial critical movement (e.g., 40 points) with the realignment's duration (e.g., 28 years). Rather, the procedures used to measure the regional impact variable must be sensitive to the fact that the realignment's force may pick up momentum in subsequent elections or lose steam. This was done by computing the difference between the status quo ante (the normal vote estimate in the year before the critical election that initiated the realignment) and each of the equilibrium points that reflect the new status quo (the normal vote estimates for every election during which the realignment endured). The regional impact measure for a region was defined only for the duration of the realignment in that region.

A hypothetical example will illustrate how the *regional impact* variable was constructed. Consider a region that experienced a critical election in 1896 as part of the Republican industrial realignment. Assume that the realignment initiated by this critical election endured in this region until it was interrupted by the New Deal realignment in 1932. The normal vote estimate for the 1892 election is .05, suggesting that it was a marginally Democratic region. Due to the events that precipitated the critical election, the normal vote estimate moved to −.15 in 1896, making it safely Republican. If that realignment was captured with a dynamic growth model, then the distribution of values for the regional impact variable could be, for example, −.20 in 1896, −.25 in 1900, −.30 in 1904, and so on. An impulse-decay model would produce the opposite type of distribution—a large initial value followed by decaying values. A step model would produce constant change throughout the 1896–1928 period. The regional impact variable for the Republican industrial realignment in this region would be defined only between 1896 and 1928.

The *aggregated regional impact* measure was calculated for each realignment era by computing a weighted average of the regional impact measures, using only regions that experienced a realignment during that era. These regional impact measures had to be weighted in order to compute the aggregated impact measure because of differences in vote totals across realigning regions. The time frame for the aggregated regional impact measure begins with the earliest critical election in the realignment era and ends when the last of the realigning regions experiences a subsequent realignment. A region's contribution to the aggregated impact measure was considered 0 if it experienced a subsequent realignment before the realignment era in question had run its course. Thus the aggregated impact measure can evidence a major decay if half of its constituent regions experience a subsequent realignment while the others do not.

The *national impact* measure was computed by weighting the aggregated regional impact measure by the proportion of the nation's vote that realigned. Thus, if the aggregated regional impact measure for a year is .40 and the realigning regions constitute half the nation's electorate, the national impact measure's value would be .20.

Electoral Impacts and Realignment Eras

The aggregated regional and national impact measures for most of the realignment eras identified earlier are displayed in Figure 3 a–g. These graphs depict the type of sharp, enduring changes in presidential electoral patterns that have captured the imagination of scholars for four decades. There is little ambiguity as to when or if a realignment begins. Moreover, the interruptions involved in critical realignments are major political happenings in the locales where they occur. While the average short-term fluctuation around a regional normal vote line is about 10 percentage points (Nardulli 1994), critical realignments result in changes that can be four to five times as great and endure for long periods of time, even if at reduced levels.

Consider the Whig realignment. It begins with an initial shift of over 55 points. This impact deteriorates gradually due to secular changes in some regions and then moves abruptly to a lower plateau in 1856, when many of the regions that realigned in 1836 realign again. The same thing happens in 1896. While the Whig realignment loses much of its influence at the regional level after 1896, many of its constituent regions do not realign again until 1932. At the national level the Whig realignment had a major impact until 1856; after 1896 its national impact was almost nonexistent.

The Republican antebellum realignment (Figure 3b) was not as sharp or as enduring as the Whig realignment and had a relatively minor impact on national electoral patterns—although not on the political life of the nation! The Democratic antebellum realignment (not displayed here) represented a sharper break than the Republican movement, but it decayed rapidly. Its electoral impact at the national level was almost undetectable. The effect of the Jim Crow realignment, reported in Figure 3c was far more enduring. It shaped voting patterns in the South for almost three-quarters of a century and forged the "solid South." In contrast to the other realignments, the Jim Crow realignment approximates the conventional, steplike, critical change so prevalent in thinking about critical realignments. While the impact of the Jim Crow realignment on national vote patterns was not great, its real national impact was felt in the electoral college. Thus its real significance is understated in Figure 3c.

The Democratic populist realignment (not displayed here) revealed a sharp break from prevailing normal vote patterns, but it decayed quickly. It never had much of a national impact because of its narrow geographic scope. The Republican industrial realignment (Figure 3d), on the other hand, began with a relatively modest 20-point shift. Its greater scope, however, translated into a significant national impact. The national impact of this interruption was all the more significant because it broke a deadlock that had gripped presidential politics since the end of Reconstruction. The industrial realignment also is important because it reveals a distinctive form. While relatively short-lived (its effects are essentially eliminated with the onset of the New Deal realignment), its impact actually picks up momentum after 1896, because of secular change. Thus it reflects a dynamic growth pattern.

The New Deal realignment (Figure 3e) also picks up some momentum after a sharp initial break. Overall, however, the New Deal era generated an electoral impact that deteriorated rather steadily after its peak. At the same time, however, its effects are felt even today. Because its geographic scope is virtually coterminal with most of the nation outside of the South, the New Deal realignment's national impact is almost equivalent to its aggregated regional impact.

Viewed more broadly, the New Deal's impact on normal vote patterns should take into account its likely role in precipitating the post–World War II realignment in the South (Figure 3f), as well as the

New Frontier realignment (Figure 3g) that followed. The initial magnitude of the post–World War II realignment was quite large, over 40 points. This movement, like the industrial realignment of 1896, evidences a dynamic growth pattern, picking up strength over the course of three decades. While its national impact upon vote patterns does not compare to the New Deal's, its real effect is felt in the electoral college, as recent presidential elections have shown. The New Frontier realignment does not reveal nearly as marked a shift as the earlier Republican movement (less than 30 points). Moreover, it deteriorates almost as soon as it peaks.

CRITICAL REALIGNMENTS AND THE STUDY OF AMERICAN POLITICAL CHANGE

The data presented in Figures 1–3 provide fairly conclusive evidence that pleas to abandon the concept of a critical realignment are ill founded. As developed and operationalized here, critical realignments are temporally structured and geographically concentrated phenomena that represent marked and enduring breaks in regional electoral patterns. At the same time, the regional realignments identified here are not the majestic national movements that many believed them to be. Moreover, as an electoral *concept*, the notion of a critical realignment does not have the sweep or appeal of the realignment *perspective* on American political change.

Does this mean that the reconceptualization proposed here has trivialized the concept? A brief review of the data presented here would suggest the contrary. The restatement offered here has simply provided a more defensible view of an extremely important electoral phenomenon. In doing so, this reconceptualization provides a much stronger foundation for a more encompassing theory of American political change. This is important because, while critics of the realignment perspective have generated important insights into the complexities of political change in the American context (Carmines and Stimson 1989; Shafer 1991), a satisfactory alternative to the realignment perspective has not emerged.

Two of the most important insights generated by these critics concern the catalytic agents that spark political change and the receptors of the transmissions that flow from disturbances created by catalytic agents. Succinctly stated, these critics contend that more catalytic agents are involved in political change than voters and that American government has become almost impenetrable by external agents such as voters. Taken on their face, these criticisms would suggest that even a resurrected conception of a critical realignment would contribute little to a broader theory of political change. This extreme interpretation notwithstanding, it is worthwhile to integrate what has been learned about critical realignments here with some of these insights concerning catalysts and receptors. Although a comprehensive theory of political change will require the development of a far more elaborate set of concepts than these two, this exercise can shed some light on the broad outlines of such a theory. It also can lead to a better understanding of the role of critical realignments in political change.

Catalytic Agents

A comprehensive theory of political change would have to incorporate a *variety* of catalytic agents. It also would have to recognize that a *hierarchy* exists among these catalysts, as well as a certain amount of *specialization*. Finally, this theory would have to recognize differences in the *contours* of the disturbances generated by these agents over time and across incidents. If a theory of political change failed to recognize these points, it would be unable to disentangle the shifting effects of the multiple forces involved in American political development.

Consider the agents of change. A disturbance can be generated by government elites (as in the neoinstitutionalist model), by social and economic leaders (as in the power elite model), by party activists (as in the issue evolution model), by organized interest groups (as in the pluralistic model), or by mass publics (as in the traditional realignment model). It cannot be expected that each will respond similarly to the same stimuli; some catalysts are more likely to be activated by certain types of issues or incidents than others. For example, party elites will be less likely to act on issues that would divide their core constituencies, or alienate important swing groups. Interest groups may be passive on important issues or developments deemed irrelevant to their raison de être.

We cannot expect that, once activated, the catalytic agents will carry the same force. At a given point in time, some are simply more powerful than others. Moreover, the hierarchy of catalytic agents has changed considerably over time. For example, in what might be called the deferential period in American politics (roughly the pre-Jacksonian period), formal governmental elites and the gentry undoubtedly were the primary movers (Heale 1977; Shallope 1990). What they felt and did mattered far more than what the masses felt or did, and there were no well established party elites or organized interest groups. Party elites undoubtedly enjoyed the upper hand during the latter part of the nineteenth century (Silbey 1991). As parties declined, party elites lost ground to better organized interest groups, more highly institutionalized governmental officials, and a more independent electorate. Currently national political parties seem to be emerging as an important force.

In theorizing about the impact of catalytic agents, it is important to differentiate between the place of a catalytic agent in the political hierarchy and the contours of the disturbance generated by a catalyst. Disturbances are complex, multifaceted phenomena. Differences in the contours of these disturbances would affect their impact on policies and institutions independent of the agent generating them. Important differences exist in mass demonstrations, lobbying efforts, and letter-writing campaigns, even when they are generated by the same agent. One need only contemplate the variance in the magnitude, scope, and duration of the realignments reported earlier to realize that not all electoral disturbances generated by mass publics are similar. But to say that realignments vary is different from saying that comparable realignments at different times produced different effects due to the electorate's placement within the constellation of forces that generate political change. Thus we must be sensitive to the contours of disturbances if we are to disentangle the effects of the multiple forces involved in the production of political change.

Receptors

In formulating a broad, encompassing theory of political change we also must pay attention to the receptors of the pulses generated by political catalysts—both representative institutions that receive the pulses and transform them into institutional and policy changes and the institutions and policy structures that absorb those transmissions. The insightful work of Chubb and Peterson (1985) stresses that these receptors have undergone important modifications over the course of American political development and that these modifications have implications for understanding political change. Consider governmental institutions:

> As the Republic has matured, its government has become further established and resistant to change. Citizens and groups have become increasingly attached to established practices, and officeholders have been ever more successful at insulating themselves—and their organizations—from external political pressures. . . . Political officials with a stake in maintaining their institutions were finding ways to insulate them from voters. Government at all levels and in all branches was employing more people divided into more departments, agencies, and committees; following more rules; and adhering more closely to established norms of behavior. Restrained internally by extensive commitments to the status quo, the political system, though responding to the rising demands of a country experiencing rapid growth and modernization, was containing these demands within increasingly well-entrenched organizations. The system was, in other words, becoming institutionalized. (Chubb and Peterson 1985, 6, 8)

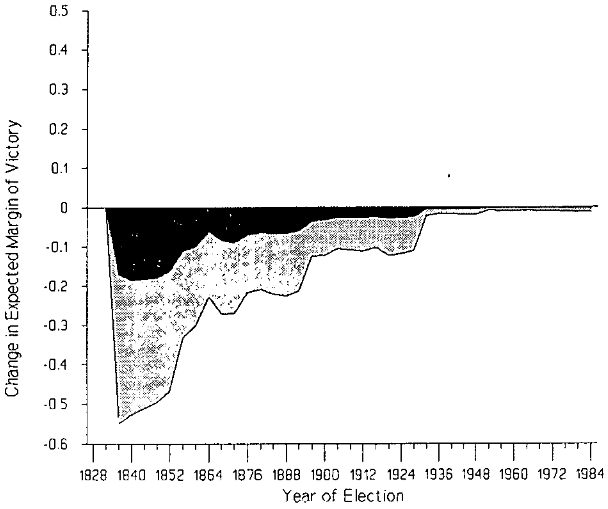
The authors go on to note that rather modest electoral shifts in 1896 led to major institutional shifts, while the major shifts in 1932 took far longer to effect institutional changes. By the 1980s, Chubb and Peterson see the engines of change within government being limited to the executive branch, national political parties, and, to some extent, the U.S. Senate. The House of Representatives and state governments largely have become conservators of past practices.

Also important are changes in the structure of certain public policy domains. In some policy domains, "programs are so complex, associated interests are so entrenched, and procedural protections

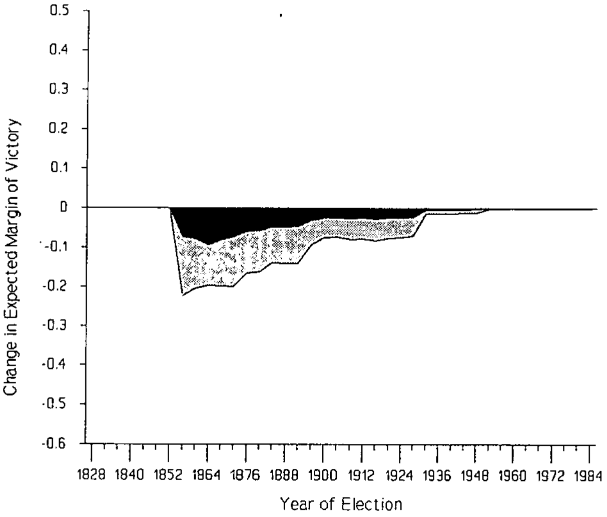
FIGURE 3

Electoral Impact of Critical Realignments

a. Whig Realignments



b. Antebellum Republican Realignments



c. Jim Crow Realignments

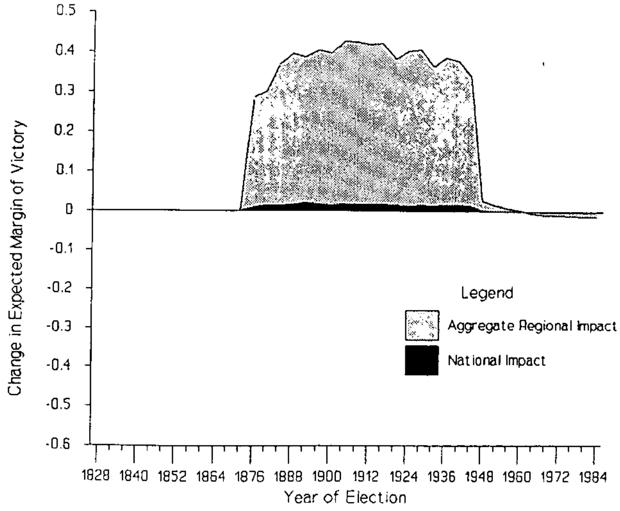
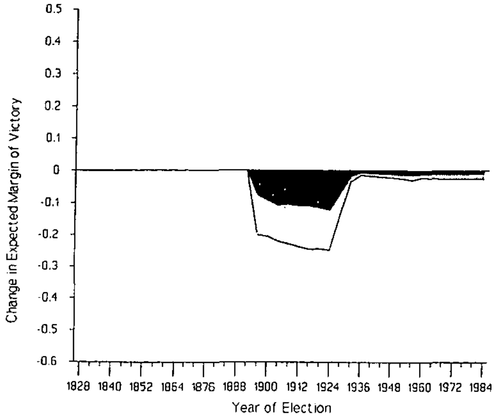
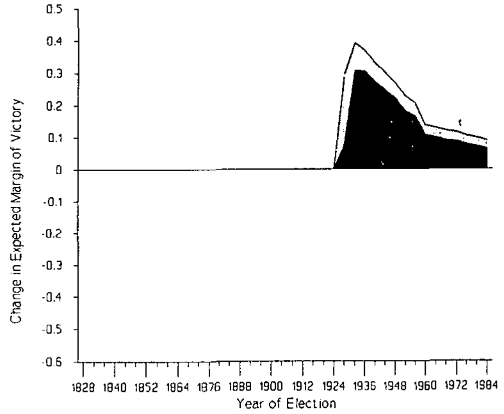


FIGURE 3 continued

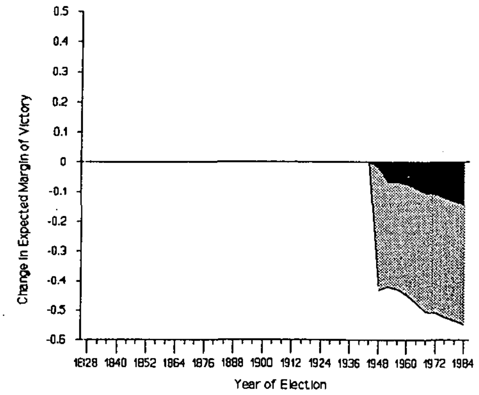
d. **Industrial Realignments**



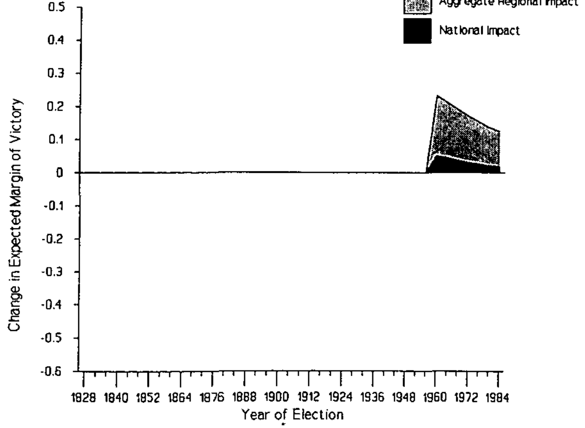
e. **New Deal Realignments**



f. **World War II Realignments**



g. **New Frontier Realignments**



are so extensive that the rate of change is inevitably slowed" (Chubb and Peterson 1985, 9). The fabric of policy in certain areas (those involving commitments to the elderly, the poor, national security, etc.) has become so tightly woven that it resists tears. Thus, while the creation of an economic recovery plan during the 1930s generated considerable resistance, that effort cannot compare to what will be required to create a new health policy.

While these observations suggest how involved a broad, encompassing theory of political change would have to be, they also make clear the role and centrality of the concept of a critical realignment. It is a disturbance generated by one catalytic agent, the electorate, among several. The significance of this disturbance to the receptors of political change will depend on its magnitude, duration, and geographic scope, as well as its reach across elective offices. Also important here, however, is the electorate's relative standing among catalytic agents, as well as whether the disturbances generated by other agents were reinforcing, undercutting, or nonexistent. How the disturbances generated by the electorate are transformed into political changes depends on the strength and clarity of the underlying message but also on the degree of institutionalization among receptors.

While the concept of a critical realignment relates to the behavior of only one political catalyst, no theory that purported to explain political change over the course of the United States experience would be complete without it. This article makes a small contribution to the development of a comprehensive theory of political change by developing and implementing an approach to capturing the disturbances generated by the electorate over the past century-and-a-half. The work reported here needs to be supplemented by the extension of this approach to other elective offices. Only then will we have a truly comprehensive measure of the disturbances generated by the electorate, a measure that will allow us to compare these disturbances across American political history. This will provide us with a good base for beginning to build and elaborate on the other components of a more comprehensive model of political change.

APPENDIX

The quantitative data reported in this article derive from a six-year study of electoral patterns in U.S. presidential elections. The first stage of this study involved the construction and refinement of a large set of electoral data—all U.S. counties and most major cities for all presidential elections between 1828 and 1984. The second phase implemented a set of procedures that grouped the counties and cities into geographically contiguous substate regions based on the similarity of their voting patterns over time. The third stage of the project derived normal vote estimates for each of these regions. The resulting data set, then, contains macro level normal vote measures for every region in the continental United States since 1828 or statehood.

The procedures used at every stage of this research were developed after a great deal of thought and pretesting, were quite involved, and were documented carefully. A detailed appendix is available from the author upon request. A less detailed overview is contained in Nardulli 1994. This appendix provides an even briefer summary of the procedures used to (1) derive the regions and (2) construct the normal vote measures. These two procedures constitute the methodological footings for the findings reported in the article.

The Derivation of Political Regions

The argument for using substate political regions as the unit of analysis for this project is developed in Nardulli 1994 and cannot be reiterated here. It rests, however, on a long line of research that focuses on the centrality of these regions in American politics since the colonial era. Despite their centrality, there has been little systematic research on the specification of these substate regions, and for good reason. To identify substate regions in a precise, reliable, and meaningful manner is a challenging task. After a great deal of preliminary analysis and experimentation, however, a set of procedures was developed to group the 3,136 counties and cities into 215 electorally homogeneous, substate political regions.

These regional groupings were identified, on a state-by-state basis, by entering the results of a cluster analysis into a mapping program that displayed the location of geopolitical units that clustered together. In the cluster analysis, counties and cities were used as the unit of analysis. The grouping variables used were a set of margin of victory (MARGIN) variables, one for each of the 40 or so elections in the 1828–1984 time period. MARGIN was defined as the Democratic percent of the total vote minus (−) the Republican percent of the total vote. The groupings of four different cluster solutions were displayed using a mapping program. Each solution was examined for reliability, geographic contiguity, and electoral distinctiveness. From these analyses one solution was chosen to represent the state's regional structure. In most states, some minor refinements had to be made to produce a geographically contiguous regional structure (i.e., 2–3 counties had to be assigned to a different grouping, about 4% of all counties).

The problem with using cluster analysis in this grouping procedure is that it will produce $n - 1$ groupings for every set of n cases on any set of m variables (Aldenderfer and Blashfield 1984; Lorr 1987). Thus, the possibility exists that the substate regions identified here are merely artifacts of the method used to identify them. That is, there may not be phenomena in the "real world" that correspond to the notion of an electorally homogeneous substate region. If there are, the methods used here may not have identified them accurately. Finally, even if the regional groupings produced here are valid and reliable, an examination of their utility in the study of electoral change must be conducted. Each of these concerns was addressed.

Face validity for the proposition that the regions identified here are "real" is found in the geographic contiguity of the counties grouped by the clustering procedure (see Nardulli 1994 for maps of electoral regions for selected states). While cluster analysis will *always* yield groupings, there is no assurance that the groupings produced would be geographically contiguous. Maps of the cluster solutions could well have appeared more like checkerboards than cohesive regional structures, but they did not.

The reliability of the procedures used here had to be checked to ensure that the results of the cluster analysis were not highly dependent upon how counties voted in a small handful of "deviant" outlier elections (e.g., 1856, 1912, 1964). To examine this possibility, a "split-half" reliability test was developed. The results of the cluster analysis for all elections were compared with the results for two other analyses that were conducted on alternating sets of elections (one group included 1828, 1836, 1844, etc.; the other group included 1832, 1840, 1848, etc.). A reliability coefficient was then computed by determining the number of counties that were assigned to the initial grouping. Overall, 88% of counties were assigned to their original grouping, a result that further suggests that the groupings are empirically well grounded.

While the results reported thus far suggest that substate regions are real and that the procedures used here do a reliable job of identifying them, we also must examine the internal cohesiveness of these groupings over time. If the counties and cities that comprise the electoral regions analyzed here do not maintain a fairly stable relationship over the forty elections that are the subject of this study, then their utility for the study of electoral change is not great. It is not reasonable to expect any group of political units to march lock-step across the panorama of American politics since the Jacksonian era, nor does the conceptual definition of a homogeneous political region require it (see Nardulli 1994). What is required is that the electoral responses of the constituent counties and cities ebb and flow together over time, in some sort of loose harmony. Stated in operational terms, we expect that the value of the MARGIN variable for constituent units will form a fairly stable band over time around the regional mean of MARGIN.

To check this expectation, we calculated the standard deviation of the MARGIN variable, by region, for each election. Thus, for each region we had up to 40 standard deviations (one for each election since 1828 or statehood) measuring the spread of the margin of victory variable for the counties and cities in the region. If these constituent units do in fact form a stable band around the regional mean across time, we would expect a fairly small mean standard deviation for the 40 or so standard deviations we have for each region. Also, we would expect those standard deviations to evidence no trend. This latter expectation is important because if, for example, the size of the standard deviations *increases* monotonically over time, it would suggest that the constituent units of the region were becoming less cohesive. Both of these expectations were met (see Nardulli 1994). Cohesiveness was especially high for regions outside the South and for elections after 1872.

Time-Series Analysis and Normal Vote Estimates

The second methodological cornerstone of this approach is the use of time-series analysis to estimate the normal vote in the regions generated by our grouping procedures. Time-series analysis is an analytic technique that is designed to facilitate the analysis of time-ordered data, which often cannot be analyzed legitimately using ordinary regression techniques (Gottman 1984; McCleary and Hay 1980). Unlike ordinary least squares, ARIMA techniques (the subset of time-series techniques that are used here) utilize a dynamic procedure that gives equal weight to all of the time-ordered observations in estimating an empirically based model of the underlying process generating the series. The ARIMA procedure is used to estimate a "noise" model that if properly constructed (stationary, homoscedastic, parameters significant and within bounds, etc.), acts as a prism in that it decomposes that data into different spectra—a deterministic component (forecast) and a noise component (residual). The decomposition of time-series data into these two spectra is ideal for our purposes because they conform nicely to Converse's (1966) conception of the normal vote.

If estimating the normal vote entailed only the construction of an adequate noise model for a given time series, it would be a relatively straightforward process. But there is more. The long-term component in electoral series does not always change slowly and short-term factors sometimes have long-term consequences. This is, of course, the thrust of the realignment perspective as developed by Key (1955) and Schattschneider (1956, 1960) and elaborated upon by a long list of distinguished scholars such as Burnham (1970), Sundquist (1983), and Clubb, Flanigan, and Zingale (1990). The existence of marked discontinuities, or interruptions, in electoral trends causes significant problems for an ARIMA analysis. If the interruptions are too large, it would be difficult, if not impossible, to construct an adequate noise model. If an adequate noise model could be constructed, its forecasts would misrepresent the origin and sharpness of the interruptions; they would appear at a later point in time. Also, they would be muted because the model would be attempting to estimate them based upon a misconception of the process that was generating them. Thus the long-term component of the process generating the time series would be misspecified, which would adversely affect estimates of the residuals.

This is wholly unacceptable because the precise identification of these interruptions is a necessary component of any attempt to operationalize the normal vote at the macro level and it is the central concern of the present analysis. To resolve this problem we must turn to a variant of time-series analysis called interrupted time-series analysis (Box and Tiao 1975; McCleary and Hay 1980; McDowall et al. 1980). Interrupted time-series analysis allows one to include in an ARIMA model a set of intervention components. The procedure tests for the statistical significance of the interruptions, to see if observations following it are likely to have been generated by a different process. Interrupted time-series analysis requires the analyst to specify the point at which the interruption occurs and the form of the change that is posited to occur. There are normally three models of temporal change that can be posited: the impulse-decay model, the step model, and the dynamic growth model. We also had occasion to fashion a fourth type that is relevant to some partisan realignments: a modified impulse-decay model.

The application of interrupted time-series analysis to the study of critical realignments presented several problems. First, realignment theory is not as precise as would be preferred in specifying when to test for realignments. For the time period under consideration, most observers would agree that, at the presidential level, major realignments occurred around the Civil War, around 1896, and around 1932. Fortunately, this ambiguity is not a significant barrier to the analysis. Not knowing whether a Civil War realignment occurred in a region in 1856 or 1860 is not as important as knowing that it occurred around that time frame. An integral part of an interrupted time-series analysis is an examination of a plot of the variable being analyzed over the time frame in question. That plot usually provides decisive information on the year a discontinuity began as well as the type of change it precipitated (impulse-decay model, step model, etc.).

Unfortunately, ambiguity concerning the hypothesized beginning of critical change is not the most serious deficiency of the realignment literature. It became clear early in the preliminary stages of the time series analysis that the literature on realignments was simply inadequate to deal with the rich complexity of electoral patterns embodied in American electoral history. This posed a serious dilemma. We did not want to begin "fishing" for realignments in a haphazard manner. Structuring a set of intervention components to fit the unique distribution of data embodied in a region's electoral history would do little to further a generalized understanding of electoral change. At the same time we did not want our efforts handicapped by conceptual blinders. It would have been irresponsible to ignore major and enduring interruptions that recurred in a significant number of regions simply because prior researchers failed to detect them. To do so would have led to serious biases in our normal vote estimates.

To deal with this problem in a methodologically responsible manner, a three-phase procedure was adopted. In phase 1, about half of the regions (130 of the 215) were analyzed. In the initial stages of this phase, traditional conceptions of when realignments occurred served as the primary guide in the specification and testing of intervention components. As insights were gained from dealing with the electoral patterns of additional regions, we experimented with additional sets of intervention components. The results of the first 130 analyses were inventoried to differentiate between idiosyncratic interruptions and those that characterized a significant number of regions, even if those regions were all in one section of the country. In phase 2, armed with this empirically grounded sense of the pattern of realignments in U.S. electoral history, the time-series analyses were reconducted for all of the initial 130 regions, as well as the remaining 85. Once all 215 analyses were completed, a graduate student who was conversant with the literature on partisan realignments and time-analytic techniques blindly examined the raw time plots of all 215 regions and was asked to specify when realignments occurred and what form they took. These "blind" specifications were compared with the results of the interrupted time-series analyses. This comparison led to about 20 discrepancies. The time series for those regions were reconducted, specifying different models of change, and the results were compared with the original analyses. This procedure led to changes in seven models.

Examples of the forecasts from this procedure are presented in Nardulli 1994, along with more details on how other problems, such as "outlier" elections (1912, 1916, 1964, etc.), were handled.

Notes

I am enormously indebted to James H. Kuklinski for many unselfish years of insightful comments on this project and this paper. My colleagues Paul J. Quirk, Michael Krassa, and Michael McBurnett provided me with important comments on earlier versions of this article, and Jeffrey M. Stonecash and W. Phillips Shively were kind enough to review the final version. This effort would not have been possible without the incredible efforts of a team of talented and devoted research assistants that included Michael Frank, Jon Dalager, Norman Hurley, and Ellen Riggle. It also could not have been done without financial support from the Research Board, the College of Liberal Arts and Sciences, the Department of Political Science, and the Institute of Government and Public Affairs at the University of Illinois.

1. Political observers have been aware of the geophysical heterogeneity of the nation and its political implications since colonial days. Turner provided the first influential scholarly treatment of the electoral and political implications of this heterogeneity (1932, 38, 45, 287–314); his pioneering work was supplemented by the later work of Key (1949) and his students (Fenton 1957, 1966; Lockard 1959). See Nardulli 1989 for a review of some key works on regionalism and politics. It must be recognized that the same type of factors that contribute to differences in partisan allegiances across regions will lead to varying reactions to the type of stimuli that lead to critical realignments. It also should be realized that while geopolitical diversity leads us to expect *different* responses to stimuli across regions, the more recent work of Huckfeldt and Sprague (1993) provide a strong theoretical foundation for the proposition that cohesive spatial units are likely to react *similarly* to stimuli that could precipitate a critical realignment. This is a key premise to the normal vote approach used here to study critical realignments.

2. The advantage of using normal vote estimates to measure the magnitude of a realignment, rather than actual vote margins, is that short-term fluctuations in the vote (noise) are removed by the time-series routine. Because a normal vote estimate is a composite of all elections in an electoral era (a set of elections demarcated by critical realignments), it provides a stable means of measuring the impact of a realignment.

3. The proportion in Figure 1 is weighted by that region's proportion of the total national vote. This was necessary because the size of the electorate varies considerably across regions. The data in Figure 1, then, show the proportion of the nation that realigned during each year. The number of regions and states will vary across years because states entered the union at different times.

4. The election of 1836 actually had three regional National Republican candidates: Hugh White, Daniel Webster, and William Henry Harrison. While the Whigs did not run a candidate under their party banner until 1840, the real break in the electoral patterns comes in 1836.

5. The inconsequential nature of the Democratic antebellum realignment depicted in Figure 1 is surprising at first glance. We would have expected more Southern regions to manifest clear, sharp breaks in their electoral patterns in the years before the Civil War. However, the combination of a critical realignment's definition and historical circumstances in the South makes this expectation unwarranted. An examination of electoral patterns in the South reveals sharp movements toward the Democrats in the early and mid-1850s, as well as a good deal of Democratic secular change beginning in the mid-1840s. However, none of the South voted in 1864, and much of it did not vote in 1868. This, of course, interfered with the emergence of stable patterns. More importantly, when the South did begin to vote in 1872 and 1876, the electorate was vastly different from the pre-Civil War period. Many regions voted heavily Republican. Because an *enduring* interruption did not emerge, many of the antebellum Democratic shifts in the South do not qualify as realignments. Instead, the entire period around the Civil War simply reveals a great deal of instability in the South.

6. This period presents special problems because a number of things are happening. First, there are a series of short-lived Republican realignments that occurred during Reconstruction. These will not be dealt with here because they are few in number, largely due to the disenfranchisement of Confederates, and because they began to deteriorate very quickly after they emerge. Second, a series of Democratic realignments emerged in New England in 1876, presumably due to the recession that began in 1873. These realignments were of some magnitude, but as they are highly localized (Massachusetts, and parts of Vermont, Maine, and Rhode Island), they will not be dealt with here. Finally, the election of 1884 saw a series of realignments in Iowa and rural Wisconsin. They also are of too little scope to merit further analysis here.

7. See n. 5.

8. The exceptions are parts of Connecticut, New Jersey, Maryland, and Massachusetts, New Mexico, and the Upper Peninsula in Michigan.

9. In addition to these three models of change, I found it was necessary to fashion a fourth—a modified impulse-decay model. This model fit electoral changes that required a series of pulses (usually two) before reaching a peak and then decaying. This adaption was necessary in order to model accurately the structure of change in urban areas beginning in 1928. Unlike most regions, most large cities registered a major shift in 1928 (immigrants reacting to the candidacy of Al Smith), followed by another in 1932. Then, like most regions that realigned in 1932, the initial interruption deteriorated. This situation was captured by measuring the 1928 jump with a "pulse" function and building it into the impulse-decay model.

10. For a graphical depiction of these various models of change using presidential elections data from the regions used in from this study, see Nardulli 1994.

References

Aldenderfer, Mark S., and Roger K. Blashfield. 1984. *Cluster Analysis*. Beverly Hills: Sage.

Almond, Gabriel. 1988. "The Return to the State." *American Political Science Review* 82:853–874.

Box, G. E. P., and G. C. Tiao. 1975. "Intervention Analysis with Applications to Economic and Environmental Problems." *Journal of the American Statistical Association* 70:70–79.

Brady, David W. 1988. *Critical Elections and Congressional Policy Making*. Stanford: Stanford University Press.

Burnham, Walter Dean. 1970. *Critical Elections and the Mainsprings of American Politics*. New York: Norton.

Carmines, Edward G., and James A. Stimson. 1989. *Race and the Transformation of American Politics*. Princeton: Princeton University Press.

Chubb, John E., and Paul E. Peterson. 1985. *The New Direction in American Politics*. Washington: The Brookings Institution.

Clubb, Jerome M., William H. Flanigan, and Nancy H. Zingale. 1990. *Partisan Realignment: Voters, Parties, and Government in American History*. Boulder, CO: Westview.

Converse, Philip E. 1966. "The Concept of a Normal Vote." In *Elections and the Political Order*, eds. Angus Campbell, Philip Converse, Warren Miller, and Donald Stokes. New York: John Wiley and Son.

Fenton, John H. 1957. *Politics in the Border States*. New Orleans: Hauser Press.

Fenton, John H. 1966. *Midwest Politics*. New York: Holt, Rinehart and Winston.

Gottman, John. 1984. *Time-Series Analysis for Social Scientists*. Cambridge: Cambridge University Press.

Heale, M. J. 1977. *The Making of American Politics*. New York: Longman.

Huckfeldt, Robert, and John Sprague. 1993. "Citizens, Contexts, and Politics." In *Political Science: the State of the Discipline*. Second Edition, ed. Ada Finifter. Washington, D.C.: The American Political Science Association.

Key, V. O. 1949. *Southern Politics in State and Nation*. New York: Random House.

Key, V. O. 1955. "A Theory of Critical Elections." *Journal of Politics* 17:3–18.

Kleppner, Paul. 1987. *Change and Continuity in Electoral Politics, 1893–1928*. Westport, CT: Greenwood Press.

Lockard, Duane. 1959. *New England State Politics*. Chicago: Henry Regnery.

Lorr, Maurice. 1987. *Cluster Analysis for Social Scientists*. San Francisco: Josey Bass.

McCleary, Richard, and Richard A. Hay, Jr. 1980. *Applied Time Series Analysis for the Social Sciences*. Beverly Hills: Sage.

McDowall, David, Richard McCleary, Erroll E. Meidinger, and Richard A. Hay, Jr. 1980. *Interrupted Time Series Analysis*. Beverly Hills: Sage.

Nardulli, Peter F. 1989. "Geo-Political Cleavages, Conflict, and the American States." In *Diversity, Conflict, and State Politics: Regionalism in Illinois*, ed. Peter F. Nardulli. Urbana, IL: University of Illinois Press.

Nardulli, Peter F. 1994. "A Normal Vote Approach to the Study of Electoral Change: Presidential Elections, 1828–1984." *Political Behavior* 16:467–503.

Schattschneider, E. E. 1956. "United States: The Functional Approach to Party Government." In *Modern Political Parties: Approaches to Comparative Politics*, ed. Sigmund Neumann. Chicago: University Press of Chicago.

Schattschneider, E. E. 1960. *The Semisovereign People*. New York: Holt, Rinehart and Winston.

Shafer, Byron E. 1991. "The Notion of an Electoral Order: The Structure of Electoral Politics at the Ascension of George Bush." In *The End of Realignment?: Interpreting American Electoral Eras*, ed. Byron E. Shafer. Madison: University of Wisconsin Press.

Shallope, Robert E. 1990. *The Roots of Democracy American Thought and Culture, 1760–1860*. Boston: Twayne Publishers.

Silbey, Joel H. 1991. *The American Political Nation, 1838–1893*. Stanford: Stanford University Press.

Sundquist, J. L. 1983. *Dynamics of the Party System: Alignment and Realignment of Political Parties in the United States*. Washington, D.C.: The Brookings Institution.

Turner, Frederick Jackson. 1932. *The Significance of Sections in American History*. New York: Henry Holt & Co.

Peter F. Nardulli is Professor and Head of Political Science, University of Illinois at Urbana-Champaign, Urbana, IL 61801-3696.

American Political Science Review Vol. 89, No. 1 March 1995

PARTY CLEAVAGES AND WELFARE EFFORT IN THE AMERICAN STATES

ROBERT D. BROWN *University of Mississippi*

*Attempts to determine the impact of party control on state welfare policy have produced mixed and inconclusive results, in part due to our inability to account for variations in the state partisan environments. I used CBS/*New York Times *surveys combined over the period 1976–88 to offer a detailed examination of the state party systems, resulting in a description of the dominant social group partisan cleavage in each state. This information is then used to examine the impact of party control on state welfare benefits. The findings show that the coalitional bases of the parties vary in important ways, both within and across the states. These differences in the state party systems have an important influence on the relationship between party control and state welfare effort. Specifically, party control has a significantly greater impact in states where partisan divisions reflect class-based New Deal–type coalitions. When examined in the context of state partisan environments, party control has a much greater impact on state welfare effort than has been suggested by previous studies.*

One of the most enduring goals of state politics scholars has been to understand the impact of political parties on policy outputs. The vast amount of research in this area is without doubt related directly to the extreme importance of this issue to our understanding of the U.S. governmental framework. If political parties, as the most prominent institutions linking citizens to the machinery of government, do not help produce policy-relevant responses, then we must question their theorized importance as linkage mechanisms and facilitators of representative democracy.

The large body of research generated to answer this question has produced decidedly mixed results. Initial failures may perhaps be described as a failure of theory, with an emphasis on interparty competition showing very little positive impact on policy outputs (Dawson and Robinson 1963; Dye 1966). Theory in this area has evolved, however, to the point where we now have a much better defined expectation of when parties may or may not matter. Rather than the level of interparty competition, or even which party is in control of government, Jennings (1979) theorized that it is differences in the constituency bases of party support that determine whether party control matters. Implicit in Jennings' theory is that parties are not likely to base their policy strategies on the preferences of the general public but, rather, on the preferences of those groups that provide the core support for the party. In Petrocik's (1981) terminology, the state party-policy linkage is dependent on the "social group bias" of the party coalitions.

Jennings' work represents a significant step in the evolution of theory linking political parties to policy outputs in the American states. Specifically, it leads to a more precise specification of the theoretical role of parties as intermediaries in state politics, one that focuses explicitly on the social group support coalitions of the parties. Our ability to examine this theory, however, has not evolved to the same degree. A significant stumbling block has been our inability to present a more rigorous specification of when we may expect to find policy-relevant differences between the parties.[1] Even Jennings' analysis was based on largely impressionistic evidence from only a handful of states. The unfortunate result is that while our theory has evolved, we are still left with largely mixed results regarding the impact of political parties on state policy.

I shall address this problem by systematically describing the party coalitions in each state. This information is then used to examine the party-policy linkage in a way that is much more consistent with the development of theory in this area. Analyzing the impact of political parties on state policy allows for the combination of two important aspects of party theory. First, I address a central tenet of party theory, that political parties in the United States are constituent organizations that organize social conflict into broad coalitions. In this view, parties are aggregates of social interests linking the mass public to the instruments of government (Burnham 1975). The coalitional bases of the parties are expected to be an important influence on the political behavior of individual partisans, party elites and, ultimately, public policy.

Focusing on the states allows for the incorporation of another fundamental aspect of the U.S. party system into the analyses. American state political coalitions have evolved in environments that vary tremendously in terms of social demography and political attitudes. Because of their nonhierarchical nature, parties and party systems at the state level have been shaped by their individual state constituencies and elites. The result is that rather than one all-encompassing party system, we have instead a national party system that is related to, but also distinct from, a set of 50 different state party systems. Each system reflects that fact that we are really a country of many different terrains in which the political landscapes can vary tremendously. Given the different ethnic and social make-up of state populations (Jewell and Olson 1988), as well as substantial differences in values, public opinion, and political culture (Elazar 1984, Wright, Erikson, and McIver 1985), state party systems should exhibit important differences in party coalitions. To appreciate the impact of political parties and party control on public policy, it is therefore imperative that we examine the nature of the party coalitions in the states.

PARTY COALITIONS AND CLEAVAGES IN THE AMERICAN STATES

Describing the State Party Coalitions

To describe the social group bases of the party coalitions, survey data are needed for the party identification and relevant social group characteristics of individuals in each state. The lack of such data has been a major obstacle in our efforts to determine the impact of political parties on public policy outputs in ways that are commensurate with the development of our theories. To facilitate these descriptions, I use CBS News/*New York Times* national polls, which, when aggregated for the years 1976–88 encompass over 170,000 respondents. These data are then disaggregated by state and treated like state samples.[2] The result is a set of 48 representative state samples that are large enough to provide reliable estimates of the group characteristics of the party coalitions in each state.

Race, religion, gender, education, income, labor union membership, and place of residence are the social and demographic characteristics that we have traditionally come to speak of when we discuss the national party coalitions. Accordingly, the following social demographic group traits are used to describe the party coalitions: place of residence (rural and urban), gender, education (college degree versus not), race (black and white), religion (Protestant, Catholic, Jewish), union membership, and income (high and low).[3]

The next step in the analysis is to measure the relative contribution of each group to the party coalitions. Unfortunately, this is not an easy task. Recent efforts by Stanley, Bionco, and Niemi (1986) and Erikson, Lancaster, and Romero (1989) take one possible approach and use multivariate logit to measure the group bases of the national coalitions (party and voting coalitions, respectively) in terms of the probability that a group member will align with either party. This type of analysis allows one to estimate, for example, the probability that someone who is Catholic will identify with the Democratic party, holding each of the other group traits of that individual constant. While this method sheds considerable light on which group characteristics may or may not promote party identification, they are less help when we describe the party coalitions themselves.

The main problem arises in the interpretation of probabilities as measures of the group contribution to the party coalition. These probability coefficients do not actually measure the group contribution to the party coalitions; rather, they constitute a model of the impact of group characteristics on party identification. This is a very important conceptual distinction. To rely on probabilities as descriptions of the party coalitions can be misleading because it fails to account for a very important point of variation in state populations. When we discuss party coalitions we need to know how group characteristics incline an individual toward a particular partisanship, *and* we need to know the relative *size* of each group in the state populations.

This distinction is important when viewed from the perspective of state party elites seeking to identify and mobilize group support at election time. To illustrate, let us look at the impact of race on party identification in Mississippi and Iowa. In each state, being black has a statistically significant impact on the likelihood of an individual being a Democrat, when all other group characteristics are controlled in a logit equation. This is just as we would expect and is consistent with the studies already cited. With regard to the party *coalitions*, however, we see that this information tells us little about the potential importance of blacks, as a group, to the Democratic party coalitions in each state. To appreciate the differing importance of blacks to the state Democratic parties, we must also consider the large difference in the proportion of blacks in the populations of these states (1% in Iowa vs. 35% in Mississippi). Given these differences, blacks will constitute a significantly greater proportion of the Democratic coalition in Mississippi than in Iowa. The result is that we may expect Democratic party elites in Mississippi to be much more attuned to the concerns of the black portion of their coalition than their counterparts in Iowa, because it is much more likely that their electoral futures will depend on this group. Thus, to describe the relevant social group bases of party coalitions, we need to know not only the inclination of the social groups to align with one party over the other (tapped by multivariate models of group identification with the parties) but also the sizes of these groups. As illustrated, this conceptual distinction has important implications for the relative size of each group in the party coalitions, and, ultimately, in the politics and policies of the states.

To account for these important social group variations in state populations I employ a variation on a technique developed by Robert Axelrod in his analyses of party voting coalitions in presidential elections (Axelrod 1972, 1986). This technique is quite straightforward, and is particularly useful because it yields figures for the social group make-up of the party coalitions that account for the general partisan inclination of each group as well as the size of each group in the population. Specifically, this technique produces figures for the percentage contribution of each group to the total party coalition based on the size of the group, its loyalty to the party, and the total number of party identifiers. The result is a formula that simplifies to

TABLE 1

Social Group Basis of the Party Coalitions Percentage Contribution of Group to Party Coalition, Selected States

	Sociodemographic Groups											
State and Party	Rural	Urban	Female	College	White	Black	Prot.	Catholic	Jewish	Union	IncLow	IncHigh
Alabama												
Dem	33	14	58	7	68	31	73	4	0	10	27	12
Rep	19	24	55	15	86	11	69	6	0	6	18	24
Vermont												
Dem	72	0	55	17	98	0	30	45	0	9	34	12
Rep	69	0	54	24	94	0	55	18	0	0	18	20
Illinois												
Dem	21	29	59	11	69	27	45	30	3	17	15	20
Rep	24	12	54	19	94	3	54	20	0	10	11	28
Arkansas												
Dem	48	7	58	9	83	16	72	5	0	8	26	13
Rep	46	5	58	9	89	10	71	4	0	3	23	15

$$\text{percentage contribution} = \frac{\text{no. of group in party}}{\text{total in party}}$$

Thus the percentage that any group comprises of the total party coalition is a function of the number of Democrats in any particular social group divided by the total number of Democrats. In other words, this represents the percentage of the Democratic coalition with a given group characteristic. Table 1 shows the percentage contribution of each group to the Democratic and Republican party coalitions in four selected states.[4]

In examining Table 1, we see that in Alabama the Democratic party receives prominent contributions from individuals living in rural areas (33%), whites (68%), blacks (31%), women (58%), Protestants (73%), and those with lower incomes (27%). Smaller contributions are made by the urban (14%), college-educated (7%), Catholic (4%), union (10%) and high-income (12%) groups. The Republican party, on the other hand, is comprised primarily of white (86%), Protestant (69%), urban (24%), and, interestingly, both income groups (higher 24%, lower 18%). Thus while both parties can claim contributions from all groups, there are differences in the percent contribution of each group to the two parties.[5] For now, it is important to gain an understanding from Table 1 that social groups do make differential contributions to the parties, both within and across the states. Comparing the Democratic party coalitions of Alabama and Vermont, for example, we find the Vermont Democratic coalition comprised of a much greater percentage of Catholics and whites and a much smaller percentage of blacks and Protestants. These differences should have important consequences for the politics and policies of these two states. Moreover, an extensive examination of the 48 states in this study reveals substantial variation in the contribution of each group to the parties across all the states. Thus the Republican and Democratic parties in the states do aggregate social groups in different ways, resulting in distinctive coalitional configurations. The social group bases of the party coalitions do vary, both within and across the states.

Describing the State Party Cleavages

The history of the national party systems leads us to expect the Democratic and Republican parties to be associated more closely with some group interests than others. In addition, differences in the sociodemographic group populations across the states are likely to result in different patterns of social group–party cleavages. Indeed, these observations have been the driving force behind the evolution of theory concerning the party-policy linkage. The major point is that given differences in the social group bases of the parties, the cleavages that define the party system in each state are likely to vary in meaningful ways. These differences are important for setting the political context in the states and thus for explaining variations in the influence of political parties on public policy.

To determine the dominant social group partisan cleavage in each state, we turn once again to the group contribution figures in Table 1. By looking down the columns for each group variable in each state, we see the differences in group support for each party. In doing so, we see that this measure of group contribution has another advantage. The percentage of the coalition figures leads to a simple yet intuitive measure of the bias in support that each group gives to one party over the other. These bias figures, shown for each state in Table 2, are simply the percent contribution of each group to the Republican party subtracted from that of the Democratic party. The result is an indicator of which groups tend to give differential support to one party over the other and the magnitude of this difference. Beginning again with Alabama, we see that the Democratic party benefits profoundly (relative to the Republican party)

TABLE 2

Basis in Group Support for State Party Coalitions

	Sociodemographic Groups											
											Income	
State	Rural	Urban	Female	College	White	Black	Prot.	Catholic	Jewish	Union	Low	High
Alabama	14	−10	3	−8	−18	20	4	−2	0	4	9	−12
Arizona	6	3	6	−11	−12	4	−19	20	2	9	7	−12
Arkansas	2	2	0	0	−6	6	1	1	0	5	3	−2
California	2	5	8	−8	−20	13	−9	8	3	8	7	−10
Colorado	−1	6	7	−5	−8	0	−16	18	1	7	3	−5
Connecticut	0	0	7	−7	−6	6	−23	19	1	7	4	−11
Delaware	2	−3	9	−5	−21	20	−7	7	1	9	10	−11
Florida	2	0	4	−9	−20	18	2	−3	3	4	9	−10
Georgia	7	−3	4	−8	−23	24	7	−5	0	2	10	−9
Idaho	−7	2	6	0	−1	−2	−15	13	0	5	7	−7
Illinois	−3	17	5	−8	−25	24	−9q	10	3	7	4	−8
Indiana	2	1	−1	−10	−12	10	−7	6	1	6	5	−7
Iowa	0	1	5	−5	−4	0	−11	17	0	10	4	−4
Kansas	−3	9	−2	−9	−9	8	−7	9	0	8	2	−2
Kentucky	0	−3	2	−3	−3	2	−3	5	0	0	−2	−4
Louisiana	9	−5	4	−8	−18	19	2	3	0	3	10	−15
Maine	2	1	9	−5	−2	0	−38	27	0	7	6	−9
Maryland	−3	8	5	−5	−23	22	−9	8	5	3	5	−7
Massachusetts	−4	2	4	−7	0	1	−27	29	2	7	6	−6
Michigan	−2	9	4	−8	−22	22	−7	8	1	10	6	−8
Minnesota	0	3	3	−7	−3	2	−12	16	0	9	7	−8
Mississippi	7	−3	−4	−8	−33	32	6	1	0	2	23	−13
Missouri	0	9	3	−6	−13	13	−7	5	0	4	4	−6
Montana	5	−4	1	−5	−3	0	−21	22	0	8	11	−10
Nebraska	−13	12	−4	−3	−6	6	−14	18	1	8	3	−4
Nevada	0	0	3	−4	−10	10	−7	8	3	7	7	−11
New Hampshire	−6	−1	13	−4	−2	2	−34	24	0	5	8	−6
New Jersey	1	6	6	−5	−21	19	−10	11	3	6	5	−9
New Mexico	1	1	12	−5	−10	1	−27	17	0	7	4	−11
New York	2	20	7	1	−23	19	−9	4	7	6	4	−5
North Carolina	5	−3	−1	0	−28	28	1	−1	0	0	7	−4
North Dakota	2	−4	12	−9	−1	−2	−12	10	0	4	2	−4
Ohio	−2	8	6	−8	−12	11	−13	12	1	8	4	−8
Oklahoma	12	−6	4	−8	−12	10	−1	−1	0	2	11	−9
Oregon	0	2	2	−7	−3	1	−10	8	0	5	2	−4
Pennsylvania	−3	12	3	−5	−12	12	−15	15	3	7	4	−4
Rhode Island	−6	11	0	−4	2	−1	−27	22	1	5	5	−6
South Carolina	16	−6	−1	−9	−41	39	−1	−2	0	0	15	−14
South Dakota	9	0	6	−6	−1	0	−16	15	0	4	5	−3
Tennessee	1	2	3	−3	−21	21	−4	−1	0	6	5	−6
Texas	7	−5	3	−13	−22	15	−3	6	0	5	10	−15
Utah	−2	0	3	−12	−6	3	−14	10	0	7	7	−6
Vermont	3	0	1	−7	4	0	−25	27	0	9	16	−8
Virginia	6	−5	0	−7	−25	25	−1	−1	1	3	4	−6
Washington	0	5	2	−7	−6	4	−9	7	1	8	3	−8
West Virginia	2	1	−3	−5	0	1	−4	4	0	3	1	−3
Wisconsin	−4	10	3	−5	−10	9	−14	9	0	6	4	−9
Wyoming	18	3	4	−3	−4	1	1	5	0	6	6	−1

Note: Cell entries represent the percent group contribution to the Democratic party minus the percent group contribution to the Republican party.

from black (20), rural (14), and lower-income (9) group support. The Republican party receives substantial differential support from the white (−18), higher-income (−12), urban (−10), and college-educated (−8) groups. Clearly, however, the highest levels of differential party support come from the racial groups.

Table 2 also shows that the pattern of partisan support is very different in Vermont. Completely gone is the differential support of the racial groups. Instead, we see a prominent role for religion (Protestant −25, Catholic 27), as well as the class-related group characteristics of income (low, 16; high, −8), college education (−7), and union membership (9).

Finally, if we turn to a state with a large, diverse population such as Illinois, we see yet a different pattern of group-party bias. Indeed, this picture represents what we have come to expect from the Democratic party as a coalition of minorities and the Republican party as a more homogeneous constituency. Specifically, the Democrats receive preferred support from the black (24), urban (17), Catholic (10), union (7), female (5), lower-income (4), and Jewish groups (3), while the Republican party benefits from the white (−25), Protestant (−9), college-educated (−8), and higher-income groups (−8).

Taken together, these bias figures represent the dominant social group partisan cleavage in each of the three states. In other words, these are the differences in the social group bases of the parties, the aspects of group support that differentiate one party coalition from another. The Alabama party cleavage is an example of what I call (for reasons that will be more apparent later) the *southern partisan cleavage*. In this party system, race plays the fundamental role in differentiating the support coalitions of the two parties. Indeed, while we see evidence of a rural-urban split, as well as differential support regarding income and education, race is without a doubt the single most important biasing factor in the group support for the parties in this cleavage.

Vermont illustrates the second prominent party cleavage, the *New Deal party cleavage*. The pattern of partisan support in these states is very similar to the national New Deal party alignment. The most prominent group-party bias exists among the two large religious groups, with Catholics and Protestants giving differential support to the Democratic and Republican parties, respectively. Next we see that the other familiar New Deal group characteristics of lower income and union membership also benefit the Democrats, whereas the Republicans receive biased support from the higher-income and college-educated groups. Once again, this is all very consistent with the more class-based New Deal party system.

Finally, we see that Illinois illustrates the third major type of party cleavage in the states. In many ways this cleavage is a combination of the previous two-party cleavages and is illustrative of the evolution of the national party system. Unlike the New Deal class-based cleavage, race is a prominent factor here. In addition, we see that the group characteristics of religion, union membership, income, and education are also important. Finally, we see the addition of a substantial urban bias toward the Democratic party. Interestingly, this is not accompanied by a similar rural bias toward the Republicans. As a result of the strong racial and New Deal-type characteristics of this party system, reflecting changes in the partisan structure of the national New Deal alignment, I have termed this cleavage the *post–New Deal cleavage*.

Examining the party coalitions in each state reveals that these three cleavage structures depict, to varying degrees, the prevailing party cleavage in 44 of the 48 state party systems examined here. In addition, there are four states (Arkansas, Kentucky, West Virginia, and Wyoming) in which there does not appear to be any discernable party cleavage. Looking at Arkansas, for example, there is a very slight bias in the two racial groups, with the remainder of the group bias figures being almost completely negligible. While one might be tempted to place Arkansas in the southern party cleavage, particularly given its proximity to the other states in this cleavage, the lack of any dominant pattern in group-party bias suggests that this is inappropriate.

States are listed in Table 3 according to their dominant party cleavage. Groups are listed under party bias categories according to the mean level of bias in each respective cleavage. Thus, in the Southern cleavage states, the two racial groups show the largest mean party bias, with the other groups following accordingly. States were categorized into party cleavages according to a careful examination of patterns in their group-party bias characteristics.[6]

It is important to note that these state party cleavages reflect the patterns of social group–party bias that have become the topic of much recent discussion regarding changes in *national* politics and the national party system. In particular, we see the varied importance of class and race as fundamental characteristics in the three types of state party cleavages. Race, for example, is by far the most important distinguishing feature of the southern party cleavage; it completely dominates the minimal role of class in the party politics of this region.

The group-party bias in the New Deal cleavage states indicates that the more class-based, New Deal party system is far from dead, at least in terms of social group alignments to the Republican and Democratic parties. Indeed, there are more states with this type of party cleavage than either of the other two cleavages. Regardless of whether the New Deal alignment adequately represents the dominant cleavage in the national party system, it appears to continue to do so for a substantial number of states. Thus, despite evidence of the rise of race and the decline of class as the basis of conflict in the national arena (Carmines and Stimson 1981; Huckfeldt and Kohfeld 1989), New Deal party system characteristics still dominate the cleavage structure in these states.

In many ways the states in the post–New Deal cleavage combine the characteristics of the first two cleavages, most notably with regard to race and class as dominant characteristics of party bias. An examination of the states listed under this cleavage reveals that in each state, class-related characteristics are important, but that race is also a dominant characteristic. In comparison with the southern cleavage, however, note that race is not necessarily the dominant characteristic of the state populations as a whole. This set of states conforms very strongly to recent evidence on the changing nature of the national party alignment. Carmines and Stimson (1981) have demonstrated that the evolution of party differences on racial issues has resulted in the emergence of this set of issues as a dominant force in American

TABLE 3

Party System Cleavage Structures

Cleavage 1: Southern		Cleavage 2: New Deal		Cleavage 3: Post–New Deal	
Bias: Democrat	Republican	Bias: Democrat	Republican	Bias: Democrat	Republican
Black	White	Catholic	Protestant	Black	White
Low Income	High Income	Low Income	High Income	Catholic	Protestant
Rural	College	Union	College	Urban	High Income
	Urban	Female		Union	College
				Low Income	
				Female	
				Jewish	

Cleavage 1: Southern	Cleavage 2: New Deal	Cleavage 3: Post–New Deal
South Carolina	Vermont	Illinois
Mississippi	Maine	New York
Alabama	New Hampshire	Michigan
Texas	Massachusetts	Delaware
Louisiana	Arizona	New Jersey
Georgia	New Mexico	Maryland
Virginia	Connecticut	California
North Carolina	Montana	Pennsylvania
Oklahoma	Rhode Island	Ohio
Florida	Minnesota	Wisconsin
Tennessee	Colorado	Missouri
	Utah	Nevada
	Iowa	Indiana
	South Dakota	Kansas
	North Dakota	
	Idaho	
	Nebraska	
	Washington	
	Oregon	

Note: Arkansas, Kentucky, West Virginia, and Wyoming do not conform to these party cleavages.

politics, leading to changes in the coalitional alignments of the national party system. Macdonald and Rabinowitz (1987) have confirmed the importance of civil rights issues at the national level as a force for structural realignment. Stanley (1988) has verified the importance of race in the coalitional alignments of parties in the South.

Finally, Huckfeldt and Kohfeld (1989) show that race has become an important cleavage force outside of the South, especially in the context of declining class cleavages. Here we find strong evidence of what Huckfeldt and Kohfeld note as the strategic importance of blacks to the Democratic party. Largely as a result of black migration to large cities in the North, the black proportion of the population, while significantly smaller than that of the southern cleavage states, has become highly concentrated and thus of strategic importance to the Democratic party (p. 13). Major urban areas such as Chicago, Cleveland, and St. Louis are all examples of areas in which concentrated numbers of blacks have created a set of structural conditions assuring the importance of this group to the Democratic party in these states. Each state in this cleavage has a similar major urban area in which blacks tend to be highly concentrated. In terms of the state party cleavages, we therefore see the emergence of race, in conjunction with the more traditional class-related groups, as the primary group characteristics differentiating the support coalitions of the post–New Deal state party systems.

PARTY CLEAVAGES, PARTY CONTROL, AND STATE WELFARE EFFORT

Having identified the dominant cleavage of partisan support in each state, I can now use this information to examine the impact of party control on public policy in a way that matches much more closely with how theory in this area has evolved. To restate this theory once again, we should not necessarily expect Democratic party control to lead to more liberal policies for the "have nots" in all states but in states where class-based interests define the most prominent cleavage in the state partisan environment. The preceding analyses of the state party systems allow us to examine this theory with systematic information about the state partisan environments for the first time. Our theoretical interest here is in the effects of partisan cleavage structures on the process of representation and public policy. Here representation refers to the relationship between party control and public policy, as conditioned by the dominant partisan cleavage in the state. Thus I am exploring how the differences across party cleavages affect the impact of Democratic party control on state welfare effort.

In generating specific hypotheses about the conditional influence of partisan cleavage differences on the party control–policy linkage, I turn first to the class-based New Deal cleavage states. If party control has an impact on state welfare policy, it should be working most prominently in these states. In addition to strong coalitional differences in income, education, and labor union support, a pronounced ethnoreligious dichotomy also exists, mirroring that found in the national New Deal party system. This strong religious differential is important, because Catholics, whether Democratic, Independent, or Republican are generally more likely to support the traditional New Deal social reforms of government intervention, government support of employment, and income equalization than their Protestant counterparts (Greeley 1978, 284). Indeed, Greeley argues that despite the rise of the social issue agenda (abortion, in particular), Catholics are likely to stay with the Democratic party, so long as the party continues in its basic stands regarding social and economic welfare (ibid., 292). The result is a party cleavage containing an important culture-class nexus in which welfare-related programs are both necessary and seen as an appropriate part of governmental activity. Once again, then, if Democratic party control does make a difference, we should expect to find it at its strongest in the New Deal cleavage states.

With regard to the other cleavages, there is certainly no reason to expect a negative effect for Democratic party control. Thus I am not suggesting that the relationship between party control and welfare effort is positive only in the New Deal states. Rather, I hypothesize that this relationship is positive, but significantly weaker in the non–New Deal cleavage states. Indeed, such a finding may well explain the rather lackluster relationship generally found between party control and state welfare effort. If the impact of party control on welfare effort is relatively meager in states where class is not a dominant aspect of the party cleavage, then the effect of these states may overwhelm even the stronger relationship between party control and policy in the New Deal states. The specific hypotheses, then, are that while the relationship between party control and welfare effort will still be positive in the southern and post–New Deal cleavage states, it will be significantly less powerful than in the New Deal states.

Data

The variables used to test these hypotheses are relatively straightforward and should be familiar to students of the party-policy debate. While several indicators of policy benefits toward the poor have been used as dependent variables, I use the policy measure suggested by Albritton (1990), namely, the ratio of indigenously raised state and local government Aid for Families with Dependent Children (AFDC) spending to total state personal income.[7]

Democratic party control is measured as an index consisting of three dichotomous indicators: whether the governor is a Democrat, whether the state House of Representatives is controlled by the Democratic party (1 if Democrats hold a majority of seats, 0 otherwise), and a similar measure of Democratic control in the state Senate. These three indicators are then used to create an index of Democratic party control running from 1 to 4. Specifically, states are scored 1 if Republicans control all three branches of government, 2 if Republicans control any two branches, 3 if Democrats control any two branches, and 4 if Democrats control all three branches.

In addition to party control, I expect that a variety of other aspects of the state political and economic environments will affect variations in state welfare benefits. Once again, these reflect some of the most prominent alternative explanations found in the literature. To control for variations in the states' ability to provide benefits to the less fortunate, I use a variation of the common measure of personal income per capita.[8] In addition, I include an indicator of the relative size of the welfare-receiving population: the percentage of the state population receiving AFDC benefits. While Plotnik and Winters (1990) found a negative relationship between this measure and the benefits given to each individual recipient, I expect that a larger percentage of recipients should be positively related to overall levels of welfare spending.

In addition to these characteristics of state populations, I include a measure designed to tap the overall liberalism of the state political environments. Specifically, I account for the general liberalism of the state party elites, using a measure developed by Erikson, Wright, and McIver (1989, 1993). This measure is a composite of the following four separate indicators of the ideological preferences of Democratic and Republican state elites, which are combined into a comprehensive measure of state elite ideology: congressional candidate conservatism-liberalism, state legislator conservatism-liberalism, local party chairmen conservatism-liberalism, and national convention delegate conservatism-liberalism.[9] Erikson, Wright, and McIver (1993) have found the liberalism of party elites to be a potent force in state politics, from responding to public opinion to influencing public policy. The expectation here is that greater party elite liberalism will be related to more generous welfare effort.

Finally, I attempt to account for one other rival explanation, the potential effects of a population's attitude toward welfare recipients—in particular, black welfare recipients. Wright (1977) reported, for example, that racial prejudice had a substantial effect on attitudes toward welfare spending independent of other measures of liberalism. In an attempt to tap this important potential influence on state welfare policy, I include a variable for the percentage of the states' population that is black.

Analysis

To examine these hypotheses, a pooled cross section (or panel) design is used, with data for the years 1976–85. Pooling cross sections of states over time has several advantages, two of the most important being an increased number of cases and the ability to generalize results across both the states and time.[10]

Unfortunately, these advantages often come with a cost. Because the design pools the same sets of units (states) over time, pooled cross-sectional models are prone to two important violations of the ordinary least squares model. Specifically, the disturbances are likely to be both nonconstant between units (heteroscedastic) and correlated over time (autoregressive). Using ordinary least squares to estimate pooled cross-sectional data is therefore likely to result in inefficient coefficients that have severely inefficient standard errors, thereby running the risk of giving a false impression of accuracy. To overcome these problems, I adopt a cross-sectionally heteroscedastic and time-wise autoregressive model for estimation (Kmenta 1986, 618–21). In this model, data are subjected to a double transformation, one to remove heteroscedasticity and the other to remove autocorrelation. The final model is then reestimated using ordinary least squares and results in consistent estimators and disturbances that are nonautoregressive and homoscedastic.[11]

Does Democratic party control have an impact on state welfare effort, and is this effect conditioned by the state partisan environment? The evidence is presented in Table 4. Note first that the familiar variables of state income and percentage of welfare recipients both exert a strong influence on state welfare effort. In addition, party elite liberalism also has the expected effect. As state elites are more liberal, welfare effort is greater. Finally, the control for the percentage of the states' population that is black also has a significant independent effect. As the black portion of the state population increases, welfare effort is diminished. Note that this is the case even with a dummy variable controlling for the South.

TABLE 4

State Welfare Effort by Democratic Party Control and State Party Cleavage

VARIABLES	MODEL ESTIMATES
Recipients as % of population	.11079**
	(.00618)
State wealth	.44006**
	(.08795)
Party elite liberalism	.000837**
	(.00009)
Percent black	−.00006**
	(.000014)
Southern cleavage dummy	.00161*
	(.00075)
Post–New Deal cleavage dummy	.00115*
	(.00047)
Party control	.00039**
	(.00012)
Southern party control interaction[a]	−.00032*
	(.00019)
Post–New Deal party control interaction[b]	−.00035*
	(.00017)
Adjusted R_2	.903
N	420

Note: Table entries are unstandardized regression coefficients, estimated with a dual transformation to remove heteroscedasticity and autocorrelation and then reestimated using ordinary least squares. Standard errors are in parentheses. The dependent variable, state welfare effort, is measured as the ratio of indigenously raised state and local AFDC spending to total state personal income.

[a] 1 if state is in southern cleavage, 0 otherwise.

[b] 1 if state is in post–New Deal cleavage, 0 otherwise.

* $p \le .05$.

** $p \le .01$.

Our primary concern, however is the impact of party control on state welfare effort, and, more specifically, whether this relationship is conditioned by the social group–partisan cleavage in the states. The coefficients for the party control and interaction variables tell the complete story and show it is important to account for differences in the state partisan environments when examining the impact of party control. Specifically, the slope for party control in the New Deal states (.00039) is significantly greater than in either the southern (slope calculated as .00039 − .00032 = .00007) or the post–New Deal (slope calculated as .00039 − .00035 = .00004) states. This pattern illustrates the main hypothesis, and it does so in striking fashion. Recall that the conditional slopes are interpreted as shifts from the baseline slope of party control in the New Deal states. That is, they represent the difference in the magnitudes of the impact of party control on welfare effort in the New Deal versus southern and post–New Deal states respectively. These coefficients indicate, therefore, that while the impact of party control in welfare effort is positive within all three cleavages, the slopes for the southern and post–New Deal cleavage states shift significantly downward from the New Deal cleavage baseline. Indeed, the coefficients shift to virtually zero and indicate that the impact of party control on state welfare effort is roughly 5.5 stronger in the New Deal states than in the southern cleavage states and an even more impressive 9.5 times greater than in the post–New Deal cleavage states. Differences in the nature of the state party coalitions have a significant influence on the relationship between party control and state welfare effort, the relationship being much stronger in New Deal cleavage states and much flatter or less responsive in both the southern and post–New Deal states.

Overall, then, I find strong evidence for the influence of party cleavage differences on the nature of policy representation in the states. Moreover, these effects stand out even when controlling for important rival forces, be they political (party elite ideology), economic (state income), or structural (percentage of recipient population). Party control has a clear impact on state welfare effort, and this effect is strongest where we would expect to see it the most—in states where the social group–partisan conflict is organized along class-related lines.

CONCLUSION

The evolution of theory on the impact of political parties on state policy outputs has led us to look toward the support coalitions of the parties themselves to understand when this linkage may be present. I have described the coalitional bases of the state party systems and the dominant patterns of partisan cleavage that result from these coalitional foundations. These investigations indicate that the social group bases of party support vary in important ways, both within and across the states.

Using this information, I find support for the conditional influence of party control on state welfare policy. Democratic party control of government matters where theory tells we should expect it to matter the most, in party systems where the dominant cleavage of social group support for the parties is drawn along class-related lines. Comparisons of class-based states with non-class-based states confirm these conclusions, showing significant differences in the impact of party control on welfare effort across these groups of states.

The literature on linkages between party control and public policy in the American states is replete with mixed and inconclusive results. By examining this linkage in the context of the state partisan environments, the findings reported here present conclusive evidence that political parties have an important influence on state policy outputs.

In addition, these findings should lead us to look once again to variations in the states to help us understand the general role of political parties in American politics. The New Deal party system may no longer be dominant at the national level, but neither has it completely disappeared from the political landscape. Importantly, the presence of this more class-based party system is not merely descriptive but also has important implications for our understanding of the role that political parties play in transmitting the preferences of partisan groups into public policy. In this important sense, variations in the social group support for the political parties add another dimension to our understanding of party control of government and the process of policy representation in the states. Whether the "party's over" (Broder 1972) at the national level or whether it has "just begun" (Sabato 1988), party systems do vary across the states. These differences are an important influence on the place of political parties in American politics.

Notes

I would like to thank Jerry Wright, Chuck Smith, Leroy Rieselbach, Ted Carmines, Jim Garand, and Jeffrey Stonecash for their help and comments on this project.

1. For interesting approaches to this problem, see Dye 1984; Garand 1985.

2. A report on the reliability and validity of these data for the years 1976–82 can be found in Wright, Erikson, and McIver 1985. The original data set has been updated to encompass the years 1976–88, resulting in a national sample of over 170,000 respondents and state samples ranging from 348 (Wyoming) to almost 16,000 (California). Alaska and Hawaii are excluded from the sampling framework.

3. Low and high income are defined as follows: 1 = low income (<10,000), 0 = else; 1 = high income (> 40,000), 0 = else. In the initial analysis, dummy variables for age were also included to account for the potential impact of younger and older social group membership on partisanship. These variables were dropped from the analysis to ease interpretation because they did little to differentiate one party from another. In addition, several other groups come to mind as being important for differentiating the support coalitions of the parties, particularly hispanics and religious fundamentalists. Unfortunately, questions asking about these group characteristics were not included in a sufficient number of surveys to warrant their inclusion here.

4. For reasons of space consideration, only the 4 states used as examples in the text are included in Table 2. The analyses were done, of course, for each of the 48 states in this study and will be summarized later in the analysis.

5. As a result of overlapping group membership, the group contribution figures do not sum to 100%. This allows for the fact that individuals have multiple group characteristics. While this may seen an inconvenience, it is not necessarily a disadvantage, especially when we consider that the party coalitions are often *defined* as coalitions of diverse, overlapping minorities. Thus, the fact that individuals do have multiple and overlapping groups traits helps make this an accurate description of the group bases of the party coalitions. Note that while an individual may get counted more than once (due to multiple group traits and thus resulting in total coalitional percentages exceeding 100%, group characteristics do not.

6. To help confirm placement of states in each cleavage category, a scoring system was developed. Cleavage scores were calculated by taking the absolute value of each group bias figure and then taking the mean of the relevant absolute value bias figures for each cleavage. Absolute values were taken to account for negativity in the Republican party bias figures, and means were taken to account for differences in the number of groups in the three party cleavages. In the southern cleavage, for example, the cleavage scores were calculated by taking the mean of the absolute values for the black, low-income, rural, white, high-income, college, and urban group bias data as follows:

Cleavage 1 = (ABSblack + ABSincomelow + ABSrural + ABSwhite + ABSincomehigh + ABScollege + ABSurban)/7.

Scores for cleavage 2 and cleavage 3 were derived in a similar fashion:

Cleavage 2 = (ABScath + ABSunion + ABSincomelow + ABSfemale + ABSprot + ABSincomehigh + ABScollege)/7

Cleavage 3 = (ABSblack + ABScath + ABSurban + ABSunion + ABSincomelow + ABSfemale + ABSjewish + ABSwhite + ABSprot + ABSincomehigh + ABScollege)/11.

In most cases the process of cleavage categorization was reasonably clear, with states exhibiting biased group party support strongly consistent with one of the dominant cleavages. In a few states, however, the patterns of group bias were not so clear, falling on the border between two cleavages. In these instances, additional information was used to place the state into one party system over the other. In Connecticut, for example, there are strong class-related bias figures, consistent with the New Deal cleavage. In addition, however, there is also a moderate racial bias, suggesting that Connecticut may also be placed in the post–New Deal cleavage. Further examination of the party coalitions shows that in post–New Deal states the average level of racial group bias is +15 for blacks toward the Democratic party and −17 for whites toward the Republican party. This is far greater than the +6 and −6 bias figures shown for the racial groups in Connecticut. In addition, the demographic data for Connecticut show that the black group comprises only 5.5% of the total state population. Once again, this is well below the average (10.9%) for the post–New Deal states. Thus, while there is some evidence of racial cleavage in Connecticut, it is the dominant overall pattern of bias in each state that determines under which cleavage a state party system is categorized. In Connecticut, this pattern is much more consistent with the New Deal cleavage.

7. Perhaps the most commonly used indicator of state welfare effort is AFDC expenditures per capita. Albritton argues persuasively that this measure is not entirely adequate because persons with larger incomes are able to make larger contributions while at the same time retaining larger net incomes. Recognizing convention, however, I performed all analyses with both dependent variables, the results being largely similar. All expenditure and income variables used in these analyses are expressed in constant 1982 dollars.

8. Because the dependent variable—ratio of AFDC spending to total personal income—includes a state-level income component in its denominator, I modify the familiar per-capita-income independent variable by expressing it in terms of national aggregate per capita income. This measure, identical to that used in a recent analysis by Hill and Leighley (1992), remains an expression of the comparative wealth of the states, while avoiding problems of extreme similarity between the dependent and independent variables.

9. Scores for party elite ideology are derived from four earlier studies of different groups of activists: congressional candidate conservatism-liberalism (Wright and Berkman 1986), state legislator conservatism-liberalism (Uslaner and Weber 1977), local party chairmen conservatism-liberalism (Cotter et al. 1984), and national convention delegate conservatism-liberalism (Miller and Jennings 1987).

10. The number of cases for the regression analysis is 420—42 states multiplied by 10 time points. The number of states is reduced to 42 because of the original exclusion of Alaska, Hawaii, and Nebraska. Nevada is excluded because measures for party elite liberalism were not available. Finally, the interactive model excludes those states that do not conform to one of the three dominant cleavages (Arkansas, Kentucky, West Virginia, and Wyoming).

11. A recent article by Beck and his colleagues (1993) addresses problems associated with panel data in which the number of units is greater than the number of time points. They discuss problems associated with one particular means of dealing with these problems—Park's (1967) method of feasible generalized least squares as discussed by Kmenta (1986, 622–25). The method used here, also discussed by Kmenta (pp. 618–21) is different from the Parks method, and provides coefficients and standard errors that are not tainted by serial correlation or heteroscedasticity.

References

Albritton, Robert B. 1990. "Social Services: Welfare and Health." In *Politics in the American States*. Ed. Virginia Gray, Herbert Jacob, and Robert Albritton. 5th ed. Glenview, IL: Scott, Foresman/Little Brown.

Axelrod, Robert. 1972. "Where the Vote Comes From: An Analysis of Electoral Coalitions." *American Political Science Review* 66:11–20.

Axelrod, Robert. 1986. "Presidential Election Coalitions in 1984." *American Political Science Review* 80:281–284.

Beck, Nathaniel, R. Michael Alvarez, Geoffrey Garrett, and Peter Lange. 1993. "Government Partisanship, Labor Organization, and Macroeconomic Performance: A Corrigendum." *American Political Science Review* 87:945–8.

Broder, David S. 1972. *The Party's Over*. New York: Harper and Row.

Burnham, Walter Dean. 1975. "Party Systems and the Political Process." In *The American Party Systems: Stages of Political Development*. Ed. William Nisbet Chambers and Walter Dean Burnham. 2nd ed. New York: Oxford University Press.

Carmines, Edward G. and James Stimson. 1981. "Issue Evolution, Population Replacement, and Normal Partisan Change." *American Political Science Review* 75:107–18.

Cotter, Cornelius P., James L. Gibson, John F. Bibby, and Robert J. Huckshorn. 1984. *Party Organizations in American Politics*. New York: Praeger.

Dawson, Richard E. and James A. Robinson. 1963. "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States." *Journal of Politics* 25:265–89.

Dye, Thomas. 1966. *Politics, Economics, and the Public: Policy Outcomes in the American States*. Chicago: Rand McNally.

Dye, Thomas R. 1984. "Party and Policy in the States." *Journal of Politics* 46:1097–1116.

Elazar, Daniel J. 1984. *American Federalism: A View From the States*. 3rd ed. New York: Thomas Y. Crowell.

Erikson, Robert S., Thomas D. Lancaster, and David W. Romero. 1989. "Group Components of the Presidential Vote, 1952–1984." *Journal of Politics* 51:337–46.

Erikson, Robert S., Gerald C. Wright, and John P. McIver. 1989. "Political Parties, Public Opinion, and State Policy in the United States." *American Political Science Review* 83:729–50.

Erikson, Robert S., Gerald C. Wright, and John P. McIver. 1993. *Statehouse Democracy*. New York: Cambridge.

Erikson, Robert S., John P. McIver, and Gerald C. Wright. 1987. "State Culture and Political Attitudes." *American Political Science Review* 81:797–813.

Garand, James C. 1985. "Partisan Change and Shifting Expenditure Priorities in the American States." *American Politics Quarterly* 13:355–91.

Greeley, Andrew M. 1978. "Catholics and Coalition: Where Should They Go?" In *Emerging Coalitions in American Politics*. Ed. Seymour M. Lipset. San Francisco: Institute For Contemporary Studies.

Hill, Kim Quaile, and Jan E. Leighley. 1992. "The Policy Consequences of Class Bias in State Electorates." *American Journal of Political Science* 36:351–65.

Huckfeldt, Robert and Carol Weitzel Kohfeld. 1989. *Race and the Decline of Class in American Politics*. Urbana: University of Illinois Press.

Jennings, Edward T. 1979. "Competition, Constituencies, and Welfare Policies in American States." *American Political Science Review* 73:414–29.

Jewell, Malcolm E. and David M. Olson. 1988. *Political Parties and Elections in American States*. 3rd ed. Chicago: Dorsey.

Kmenta, Jan. 1986. *Elements of Econometrics*. 2d. ed. New York: Macmillan.

Miller, Warren E., and M. Kent Jennings. 1987. *A Longitudinal Study of Party Elites and Party Supporters*. New York: Russell Sage.

Parks, Richard. W. 1967. "Efficient Estimation of a System of Regression Equations When Disturbances Are Both Serially and Contemporaneously Correlated." *Journal of the American Statistical Association* 62:500–509.

Petrocik, John R. 1981. *Party Coalitions: Realignment and the Decline of the New Deal Party System*. Chicago: University of Chicago Press.

Plotnik, Robert D. and Richard F. Winters. 1990. "Party, Political Liberalism, and Redistribution." *American Politics Quarterly* 18:430–58.

Sabato, Larry J. 1988. *The Party's Just Begun*. Glenwood: Scott, Foresman.

Stanley, Harold W. 1988. "Southern Partisan Changes:

Dealignment, Realignment, or Both?" *Journal of Politics* 50:64–88.

Stanley, Harold W., William T. Bianco, and Richard Niemi. 1986. "Partisanship and Group Support Over Time: A Multivariate Analysis." *American Political Science Review* 80: 969–78.

Uslaner, Eric M., and Ronald E. Weber. 1977. *Patterns of Decision Making in State Legislatures*. New York: Praeger.

Wright, Gerald C. 1977. "Racism and Welfare Policy in America." *Social Science Quarterly* 57:718–30.

Wright, Gerald C., and Michael B. Berkman. 1986. "Candidates and Policy in United States Senate Elections." *American Political Science Review* 80:567–88.

Wright, Gerald C., Robert S. Erikson, and John P. McIver. 1985. "Measuring State Partisanship and Ideology with Survey Data." *Journal of Politics* 47:469–89.

Robert D. Brown is Assistant Professor of Political Science, University of Mississippi, University, MS 38677.

American Political Science Review Vol. 89, No. 1 March 1995

SHAPING MEXICO'S ELECTORAL ARENA: THE CONSTRUCTION OF PARTISAN CLEAVAGES IN THE 1988 AND 1991 NATIONAL ELECTIONS

JORGE I. DOMÍNGUEZ *Harvard University*
JAMES A. McCANN *Purdue University*

In the 1988 and 1991 national elections, Mexican voters asked themselves above all whether they continued to support the long-ruling official party. Voter behavior was not well explained by attachments to social cleavages, attitudes on policy issues, or general assessments about the present circumstances and the prospects for the nation's economy or personal finances. In both elections, moreover, the parties of the Left failed to mobilize voters that had chosen to abstain in past elections. Once voters were ready to oppose the ruling party, however, differences by issue, prospective economic assessments, and social cleavages shaped their choice between the opposition parties.

How do citizens who have long been governed by the same party in an authoritarian regime vote when there seems to be a chance that they could turn the incumbents out of office? In such elections, do citizens focus mainly on the future of the ruling party and its incumbents? Are these elections ideologically competitive, with party choice shaped by attitudes on policy issues and social cleavages? How do such citizens respond to the entry of a new party to challenge the rulers, and how strong is such a new party likely to remain from one election to the next? How stable and distinctive are blocs of voters once the electoral arena becomes more competitive? We seek to address these questions by focusing on the 1988 and 1991 national elections in Mexico.

In the July 1988 presidential elections, Mexico's electorate split into three large voting blocs (Table 1, see the note). The long-ruling Institutional Revolutionary Party (PRI) claimed just over half the votes for its presidential candidate, Carlos Salinas de Gortari. (In 1982, the PRI and small allied parties had claimed 71% of the votes cast for its presidential candidate, Miguel de la Madrid; in the 1960s and 1970s, the PRI and its allied parties always claimed over 80% of the votes cast in presidential elections.) The long-established opposition party, the National Action party (PAN), came in third, well behind a new political force, a coalition of several parties on the political Left led by Cuauhtémoc Cárdenas. Cárdenas himself, and many of his close associates in the new coalition, had once belonged to the PRI. In the August 1991 national congressional elections, however, the PRI's recorded votes soared and the PAN held steady, but the Cárdenas coalition splintered. The party that remained formally associated with Cárdenas, the Democratic Revolutionary party (PRD), received just over 8% of the recorded votes. Seven minor parties, the largest of which were former Cárdenas allies, each garnered from .6% to 4.4% of the recorded votes cast, for a total recorded vote of 12.7% for the minor parties.

We hope to show that the voting intentions of Mexican citizens as reported to our pollsters are best explained in terms of voter judgments about the prospects for the ruling party and the voter's past party preferences. These explanations are strongly significant statistically and hold for both national elections. Voters ask themselves, above all, whether they continue to support the ruling party—the *party of the state* as Mexican scholars sometimes call it. Contrary to the views of many commentators on these Mexican elections (especially on the 1988 election), we also find that voter behavior is not well explained by (1) attachments to social cleavages, (2) attitudes on policy issues, or (3) general assessments about the present circumstances and the prospects for the nation's economy or personal finances.

We shall also argue that the organizational weaknesses among the parties of the Left help to explain important aspects of the outcomes in both elections. In 1988, the Cardenista coalition failed to mobilize voters who had chosen to abstain in past elections; in 1991, the Cardenista coalition splintered even though the pool of potential Cardenista voters remained large. That is, from 1988 to 1991 opinion alignments remained more stable than party alignments. Further, we suggest that the explanations for voting behavior in 1991 differ from those that obtained in 1988. In 1988, opposition voters were motivated by a range of political, social, and economic concerns; by 1991, opposition voters were motivated almost exclusively by their political judgments and by their place of residence: instead of national parties, the opposition parties had shrunk into regional parties. Finally, we shall propose and explain a two-step "model" of Mexican voting behavior. Mexicans decide first on their judgment about the ruling party; lurking beneath that judgment, especially in 1988, differences by issue, prospective economic assessments, and social cleavages shape voter choices between opposition parties and, therefore, the electoral arena.

TABLE 1

Partisan Preferences in Mexico, 1988 and 1991

	Percent of Vote
1988 Presidential Election	
Institutional Revolutionary party (PRI)	55
National Action party (PAN)	20
Cardenistas	22
Minor parties	2
1991 Legislative Elections	
Institutional Revolutionary party (PRI)	68
National Action party (PAN)	15
Democratic Revolutionary party (PRD)	7
Minor parties	9

Source: Gallup interviews conducted in May 1988 and July 1991.
Note: Respondents without a clear partisan preference have been dropped from this list. N = 1,914 (65% of the total sample) in 1988 and 2,029 (66%) in 1991. In 1988, the official tally gave 50% of the vote to the PRI, 17% to the PAN, 32% to the Cardenistas, and 1% to all other minor parties combined. In 1991, 61% of the vote went to the PRI, the PAN gained 18%, the PRD received 8% and the other minor parties polled 13%.

ASSESSING ARGUMENTS

The Institutional Revolutionary party (PRI) has long been a bureaucratic organization bearing its contradictions in its own name. The party claims legitimacy from both the Mexican revolution and its near opposite, institutional rule (Bailey 1988; Basáñez 1982). We shall now spell out the reasoning for our choice of independent and dependent variables.[1]

The Ruling Party

When citizens long governed by the same party in an authoritarian regime face a fairly open election, the fate of the ruling party becomes the central question for many voters; even those who oppose the ruling party do not necessarily oppose all of its policies, nor does their vote embody the social cleavages often evident in competitive politics (Huntington 1991, 174–92). We assess the importance of ruling-party factors through questions that ask respondents about their partisan loyalties and about the nation's prospects if a different party were to govern. If Mexico's voters were not focused mainly on the fate of the ruling party, then attitudes toward specific issues, general expectations about present and future economic circumstances, and ties to social groups would have a more salient role, while views about the ruling party's future would matter much less. To distinguish between these hypotheses, we take into account the Mexican context.

First, in Mexico, questions pertaining to, say, freer trade or foreign investment do not a priori tap support or opposition for the ruling party because the leaders of one major opposition party, the PAN, have officially favored such general policies. Responses to these questions thus measure attitudes on specific issues, not necessarily attitudes toward the ruling party. If attitudes toward issues were to explain Mexican voting behavior, we would conclude that the hypothesis about the preeminence of ruling-party factors in explaining voter choice does not hold.

Second, questions about prospective economic circumstances help to explain voter choice in many European countries where the same party has not governed alone for generations (Lewis-Beck 1988). We distinguish between responses to questions about the prospective economic situation posed in general terms from questions about the country's prospective situation that are specifically related to what would happen if a party other than the PRI were to gain power. If general questions about economic expectations explain Mexican voting behavior, we would conclude that the hypothesis about the preeminence of ruling-party factors in explaining voter choice does not hold.

Third, as the party of the state until 1988 by means fair and foul (at times including coercion and fraud), the PRI claimed election victories virtually without fail (Gómez Tagle 1989, 240–242). The PRI has never accepted defeat in a presidential election. It had never accepted defeat for a Senate seat until 1988, or for a governorship until 1989. The PRI had been integral to the authoritarian regime. Asked about a future without the PRI in power, Mexicans understand that they are not being asked about the fate of a "mere party." We asked voters whether the country's economic conditions or social peace would be hurt or helped if a party other than the PRI were to gain power; we also asked about general expectations concerning PRI strength. If these questions were to prove insignificant—or less important than questions about demographic factors, issues, or general economic expectations—in explaining voter choice, we would conclude that the hypothesis about the preeminence of ruling-party factors in explaining voter choice does not hold.

The Organization of the Ruling Party

Before the 1988 elections, PRI organization typically rested on collective affiliation, not individual membership. Various organizations belonged to the three "sectors" (one each for workers and peasants and a third more heterogeneous one for middle-class organizations) that have constituted the PRI since its major reorganization in 1938. Party leaders used this internal party organization to channel support and to manage conflicts. Paradoxically, this practice left the PRI without members: it sought the support of organizations but not directly of the nation's citizens (Garrido 1987).

The impact of this PRI internal structure on voting behavior was open to competing hypotheses. Did the PRI have a strong hold on individuals affiliated with the party in this indirect manner or did the indirect affiliation weaken individual loyalties to the party, rendering once PRI voters vulnerable to the appeals of a new opposition party, many of whose leaders had once belonged to the PRI? The PRI had long drawn strength from its peasant and labor unions whose members might be expected to vote PRI. Alternatively, the indirect nature of PRI "membership," the weakness of an internal party life, and some labor and peasant leaders' reputation for bossing their members might mean that lower-class voters had no special attachment to the PRI. As early as the 1960s and 1970s, there had been work-center revolts against traditional PRI-affiliated labor unions (Middlebrook 1989). This disconnection between the PRI and its presumed affiliates might cancel the effects of demographic factors. Under this alternative, voting would be unrelated to union membership, social class, religion, region, and other demographic factors.

The Presidency and the Issues

Mexico's political system has been highly centralized; the presidency has been "at its epicenter" (Bailey 1988, 30). The president has always come from the PRI since the party's foundation. We would be shocked if presidential performance were not to explain much about voting behavior in Mexico.

In the 1980s, Mexico's economy was in deep trouble. From 1981 to 1989, Mexico's gross domestic product (GDP) in constant prices per capita fell by over 9%; GDP per capita change was negative in five of those years. Consumer price inflation had been below 30% per year in the late 1970s and early 1980s; it was well above 60% per year every year from 1982 through 1987, when annual inflation peaked at 159%; the inflation rate in 1988 was 52%. In 1988, the minimum salary in real terms in Mexico city was 46% below its 1980 level. In 1988, the average salary in Mexican manufacturing was 28% below its 1980 level (United Nations, Economic Commission for Latin America and the Caribbean 1989).

From 1989 through 1991, gross domestic product per capita grew every year (on average, 1.7%). In each of those three years, inflation was below 30% per year, with an overall downward trend. The average salary in Mexican manufacturing recovered slightly, but in 1991 it was still 23% below its 1980 level. The minimum salary in real terms in Mexico city continued to drop; in 1991, it was 57% below its 1980 level (United Nations 1991).

The de la Madrid administration made two key economic policy decisions: it continued to service the foreign debt and to negotiate its rescheduling with its creditors, and it shifted away from the long-standing policies of industrialization by means of import substitution and heavy state involvement in the economy toward a market-oriented strategy and export promotion, reducing the state's impact on the economy. The Salinas administration accelerated these trends. It reached a comprehensive international debt settlement with its creditors, privatized many state enterprises, and removed many barriers to (and sought to promote) direct foreign investment. It greatly liberalized international trade and created a North American free-trade area with the United States and Canada. Many PAN leaders who had advocated similar policies supported the government's born-again economic liberalism, but these policies helped to galvanize the Cardenista opposition.

In 1988, therefore, many voters had reason to worry about the country's and their own finances. In 1991, most Mexicans had yet to gain much from the economic recovery that was under way, even if some could have bright hopes for the nation's and their own economic future.

In terms of economic policy orientations, the speeches of politicians in 1988 and 1991 suggested some convergence between the PRI and the PAN in support for freer markets but also indicated that voters who disagreed with those policies could vote for Cárdenas. In 1988, for example, Cárdenas argued: "I think that this administration has been letting foreigners take over our fundamental decisions. It has acted not in the interest of the country but of foreigners who are against Mexico"; again: "Possibly, the present administration and the program of Carlos Salinas are rational in some way, but it is a rationality which goes against the majority. It is a rationality that has made many Mexicans poor, that leaves important economic decisions to foreigners, that closes the channels of democratic expression. A rationality conducive to repression and dictatorship" (WGBH Educational Foundation 1988). Although some policy differences were more muted by 1991, Cárdenas remained a clear alternative to the Salinas economic policies. Issue-oriented voting was certainly rational and conceivable in both 1988 and 1991. Indeed, commentators during both campaigns frequently portrayed the electoral choice in stark ideological terms.

The Opposition, Cleavages, and Parties

Did the shaping of Mexico's electoral arena in 1988 and 1991 conform to broader patterns of the construction of partisan cleavages? As Lipset and Rokkan (1967) argued in their classic work, in the genesis of party systems there are often territorial and religious bases for opposition because of a clash between the central nation-building political coalition and territorially or religiously defined oppositions. In Mexico, the PRI markedly centralized power in the capital city and, at moments in its history, has sponsored anticlerical policies.

The PAN might embody these territorial and religious cleavages. Although the PAN had had a long, if generally unsuccessful, electoral trajectory opposing the PRI, it has been a real political party, with its own members, activists, programs, and goals. Especially in some northern states such as Chihuahua, there was a genuine vibrancy to its internal life (Chand 1991). It may have won the 1986 gubernatorial elections in Chihuahua (though the official results gave the governorship to the PRI), and it won the Chihuahua governorship at last in the 1992 elections. In the PAN's history, fidelity to Roman Catholicism had played a significant role (Almond and Verba 1965, 93; Loaeza 1989, sec. 3). The PAN has also seemed stronger among middle-class voters. Thus we expected individual voter loyalty to the PAN in election after election, plus region, church attendance, and social class to help to explain voting for the PAN (Butler, Pick, and Jones 1991).

The Cardenista coalition was new in 1988. Formed around the person of Cuauhtémoc Cárdenas, a former PRI governor of the state of Michoacán and son of former President Lázaro Cárdenas, Cardenistas lashed out at the PRI's lack of internal democracy—at such practices as having the incumbent president de facto "appoint" his own successor or having most PRI candidates for governor and Congress be chosen by a few leaders or by the president himself. We expected Cardenistas to oppose the PRI, the official candidate, import liberalization, the sale of state enterprises, and continued servicing of the foreign debt. Consistent with the Lipset–Rokkan argument, the Cardenista coalition had some features of a socialist party in part because, in 1988, it included various parties that bore the name *socialist* and because, in both 1988 and 1991, it included the old Mexican Communist party. The Cardenista party's newness led us to ask how they got their votes. Did they mobilize the previously unmobilized? Did they take voters, as well as leaders, from the PRI? Had PAN voters in the past been "closet leftists" who had voted for the PAN mainly because there was then no viable left-wing alternative?

By the 1991 congressional elections, the Cardenista coalition had splintered into many of its initial components. Did this fragmentation reflect policy differences in the electorate or differences attributable to demographic factors? Did the collapse of the broad Cardenista coalition reflect factional infighting? If the latter, the voters for Cárdenas' own PRD in 1991 should resemble the voters for the minor parties in their concerns about the ruling party, the presidency, and the issues and in their demographic characteristics.

THE DATA

This study is based largely on two nationwide public opinion polls, both conducted by the U.S. Gallup Organization (Princeton, New Jersey) in collaboration with its Mexican affiliate, Gallup México (known as IMOP S.A.). Sampling, questionnaire design, and overall direction were U.S. Gallup's responsibility; IMOP's personnel, long experienced in polling Mexicans, did the field work.[3] The first poll was conducted from 12 May until 1 June 1988, carrying out a total of 2,960 personal interviews.[4] The second poll was conducted from 15 through 28 July 1991, carrying out a total of 3,053 personal interviews.

In both instances, each interview lasted on average about 40 minutes; the interviewees were all 18 years old (Mexico's voting age) or older. Mexico was divided into four regions (North, South, Mexico City Federal District, and Central Mexico minus the Federal District). The distribution of interviews was proportionate to each region's population. Cities, towns, and villages (with at least 1,000 residents) designated as polling locations were chosen randomly within each region; there were 78 such locations in 1988 and 270 in 1991. Within each polling location, blocks, households and respondents in the household were selected at random, with about equal numbers of men and women. The margin of error attributable to sampling error is ±3% in 1988 and ±2% in 1991.

The 1988 poll ended five weeks before the presidential election. During those weeks, the support for Cárdenas grew dramatically (see Table 1 and its note). Many observers believe that the vote for Cárdenas was in fact higher. (The final result of the "real" election is neither known nor knowable, the ballots having been destroyed.) This discrepancy does not mean that the poll was in error. The poll probably reflected accurately the state of opinion in the second half of May, but the state of opinion changed (Basáñez 1992). In any highly charged campaign environment with a new party in the making, this is to be expected.

The 1991 poll ended three weeks before the congressional elections. In the official results, the PRI was credited with less support than the Gallup poll had found (see Table 1 and its note). The results for the PAN, the PRD, and the minor parties are fairly close between the Gallup poll and the recorded votes. As we shall see, there was a surge for the minor parties in the days just before the election.

In the case of both polls, there is a likely overestimate of the support for the PRI beyond the formal comparison between the polls and the respective official results. In both cases, some who told Gallup's pollsters that they would vote for the PRI did not, in fact, do so. It is impossible to use the poll to discern who changed their minds or who may have lied. The effect of these difficulties on our analysis is that our measure of support for parties other than the PRI is quite conservative. It identifies those who did not fear to tell the pollsters about their preferences. In the case of Cardenistas in 1988, these were the coalition's founders.

The design of each poll took into account the expectation that some voters would be reluctant to express their true preferences. Thus, in 1988, the question with regard to presidential candidate preference was phrased as follows: "Now, just for the sake of this study, let us imagine that today is election day and you are going to vote for the next President of the Republic. What you indicate will be completely confidential and will only be used for this poll. On this sheet (a sheet is handed out) please mark the political party for which you intend to vote for President of the Republic and deposit it in this box." Then a box is presented to attempt to convey the sense of confidentiality. In the 1991 poll, the same procedure was used to foster belief in, and to ensure, confidentiality (voters being asked to choose a party). In both years, the simulated secret ballot procedure was repeated for several questions deemed to be potentially controversial. We have only used questions asked via "secret ballot" to measure the dependent variable,[5] and we are fortunate that so many questions in these surveys were asked in this fashion.

DESCRIBING THE ELECTORATE

Our description of Mexico's electorate is summarized on Tables 2–4. The Appendix presents the questions in the sequence to which reference is made in these tables; the same sequence is repeated in Table 5. There was a very substantial increase in support for the PRI from 1988 to 1991; this increase is found in each and every category (see Tables 1 through 4). We have no doubt that President Salinas impressively turned around his party's fortunes during those three years.

Demographic factors delineate clusters of voters, although most relationships between demographic factors and party preferences are not very strong. The PRI drew more support from women, heavily rural southern Mexico, older voters, union members, the less well educated, and church-goers. The PRI did consistently less well in the Federal District. These findings hold for both elections. The PRI comes through as a "traditionalist" party. The only demographic change in the PRI's basis of support from 1988 to 1991 was its recapture of strong middle-class support after having yielded more than half of the middle-class voters to the opposition in 1988. Even in 1988 the PRI got nearly half of the youngest voters, while in 1991 it got a majority of those voters and was strongest overall among voters aged between 26 and 29 years. At a time of economic duress, the PRI's ability to corral these voters was impressive.

The PAN drew stable support in the North and more variable support in Mexico City. It had more support among the youngest voters, the somewhat better educated, and those who did not belong to unions. Gender and religion did not explain much of the support for the PAN. The consistent lack of relationship between church attendance and PAN support is surprising. In 1988 the PAN did less well with working-class voters, but in 1991 it improved its performance with these voters while it lost voters

TABLE 2

Demographic and Social Determinants of Partisan Choice (%)

DEMOGRAPHIC AND SOCIAL CHARACTERISTICS	1988 PRI	1988 PAN	1988 CARD.	1988 *N*	1991 PRI	1991 PAN	1991 PRD	1991 MINOR	1991 *N*
Gender									
Male	53	20	25	973	63	16	10	11	975
Female	58	21	20	941	74	15	5	7	1,054
Region									
North	59	24	15	479	71	21	5	3	477
Central	56	17	24	474	72	15	6	7	532
South	67	12	20	446	78	6	4	11	421
Federal District	42	25	29	515	56	18	13	14	599
Age									
18–25	49	21	26	510	59	20	9	12	534
26–29	50	23	24	231	75	14	6	5	210
30–39	56	20	23	518	66	14	14	6	608
40–49	60	20	19	330	71	12	7	10	363
50+	64	17	17	325	74	14	6	6	334
Union Member?									
Yes	64	14	22	442	74	13	7	5	460
No	53	22	23	1,472	67	16	8	10	1,509
Class									
Professional	58	21	18	324	71	14	9	6	294
Middle	46	21	29	298	70	14	6	10	272
Working	59	15	24	325	62	17	10	11	386
Education									
Primary	63	17	18	677	73	11	7	9	792
Secondary	56	18	25	415	67	19	6	8	442
Preparatory	47	24	27	400	62	17	9	12	380
University	51	23	23	415	66	18	8	8	374
Church Attendance									
Weekly	59	21	18	953	70	16	6	8	925
Occasionally	54	19	25	793	69	14	8	9	966
Never	45	22	33	163	52	15	18	15	126

Source: Gallup interviews conducted in May 1988 and July 1991.

TABLE 3

Partisan Choice by Economic Perceptions and Policy Preferences (%)

ECONOMIC PERCEPTIONS AND POLICY PREFERENCES	1988				1991				
	PRI	PAN	CARD.	*N*	PRI	PAN	PRD	MINOR	*N*
Economic performance									
Current									
Good	66	16	16	462	73	13	6	8	1,071
Bad	52	22	24	1,404	63	18	10	10	907
Future									
Improve	68	12	19	465	73	15	5	7	937
Worsen	45	27	26	674	67	16	10	8	572
Personal financial situation									
Current									
Good	59	20	20	1,109	71	14	7	8	1,386
Bad	50	21	26	757	64	17	10	10	630
Future									
Improve	58	18	22	877	69	15	7	9	1,457
Worsen	52	24	23	461	67	14	9	11	239
Foreign investment									
Positive	57	21	21	1,065	72	16	5	7	1,292
Negative	50	20	27	563	61	15	13	12	452
Both	50	19	26	114	56	19	12	14	146
Imports									
Made easier	56	22	21	950	71	16	6	7	1,158
Limited	53	19	25	847	62	17	10	12	632
Continue paying foreign debt?									
Yes	58	20	20	1,335	—	—	—	—	—
No	47	21	29	477					
Retain state industries?									
Yes	56	16	25	854	—	—	—	—	—
No	52	25	21	859					
Environmental Protection									
Most important					68	15	8	9	1,218
Moderate importance	—	—	—	—	70	17	6	8	583
Not important					66	14	8	13	192

Source: Gallup interviews conducted in May 1988 and July 1991.

from the middle and professional classes. Except for this social class switch and the greater concentration of support for the PAN in the North in 1991, its demographic profile was stable between the two elections.

In 1988, the Cardenista coalition drew support from men (the gender gap widened from 1988 to 1991), younger voters, the middle class, the better-educated, and those who never went to church. (The latter were nearly twice as likely to vote Cardenista as those who went to church every week.) Its support was strongest in Mexico City and weakest in the North. Union membership did not matter. In 1991, the PRD had become a regional party, strong in Mexico City. It had lost support among the middle class. (In 1988, its middle-class support had exceeded its working-class support.) Support for the PRD was more evident among men, in the working class, and among those who did not go to church. Age, education, and union membership did not explain much.

In 1991, support for the minor parties resembled support for the PRD. The minor parties, too, drew disproportionate support from men, from the working class, and (strongly) from those who did not go to church. Age and education did not explain much in a systematic way. As with the PRD, the minor parties were stronger in Mexico City. Unlike the PRD, the minor parties were also strong in southern Mexico and among union members.

Turning to views on general economic assessments and issues (Table 3), support for the PRI was not surprisingly higher among those who thought that Mexican economic performance had been good and would get better and that their personal finances were good and would also get better. Support for the opposition was stronger among the dissenters. These differences were notable in 1988 but narrowed markedly by 1991.

From 1988 to 1991, the PRI found more supporters among backers of foreign investment and freer trade. In 1988, support for the PRI was higher among those who favored servicing the foreign debt and somewhat so among those in favor of retaining state enterprises. In 1991, support for the PRI seemed unrelated to views on environmental protection.

Support for the PAN was unrelated to economic issues in both years. PAN supporters were found in comparable proportions among those who supported

TABLE 4

Partisan Choice by Political Interest, Prior Voting Behavior, and Perceptions of the PRI (%)

POLITICAL INTEREST, PARTISAN HISTORY, AND PARTY PERCEPTIONS	1988				1991				
	PRI	PAN	CARD.	*N*	PRI	PAN	PRD	MINOR	*N*
Interest in politics									
Much/some	56	20	22	809	69	16	9	7	693
Little/none	55	20	23	1,092	68	15	7	10	1,325
Prior Pres. Voting									
PRI	76	9	13	962	93	3	2	3	969
PAN	12	60	28	270	16	72	6	7	198
Cardenista					22	6	38	34	152
Not mobilized	51	20	28	240	67	17	7	9	377
PRI stronger or weaker in 10 yrs.									
Stronger	73	12	14	619	79	11	5	6	946
Same	64	17	18	470	69	15	7	10	494
Weaker	34	30	34	669	42	29	15	14	432
If another party wins									
National economy									
Better	31	30	36	749	47	27	14	12	565
Same	64	18	16	525	71	12	7	10	664
Worsen	82	9	8	396	87	7	3	4	542
Increase in Social Unrest									
Yes	61	18	20	1,068	75	13	6	7	1,151
No	45	25	28	683	57	20	10	13	698
Presidential Approval Rating									
Very high	79	10	11	364	81	11	4	4	971
Favorable	62	17	19	721	60	17	8	14	757
Mediocre	38	30	30	473	40	26	21	13	192
Poor	26	33	37	206	59	16	14	10	49

Source: Gallup interviews conducted in May 1988 and July 1991.

and opposed foreign investment, who supported or opposed trade liberalization, and who would or would not service the foreign debt. PAN supporters in 1991 were comparably split over their views on environmental protection. In 1988, PAN supporters did favor privatizing state enterprises. In both years, PAN supporters thought that the economy's performance was bad and its prospects not good, but the differences narrowed by 1991. PAN supporters were more closely divided (though, on balance, pessimistic) in both years on their views about their personal finances; these differences also narrowed by 1991.

Support for the Cardenistas in 1988 was also only weakly related to specific issues. By modest margins, support for the Cardenista coalition was higher among those who held negative views on foreign investment, freer trade, servicing the foreign debt, and privatizing state enterprises. In 1991, however, the smaller pool of PRD supporters was much more likely to be found among opponents of foreign investment and freer trade—an attitude profile that once again matches that of the minor parties. In 1991, PRD supporters had indistinct views with regard to environmental protection but the supporters of minor parties were more likely to be found among those who thought that this was not an important issue. In both years, Cardenistas believed that the economy's performance, their own financial circumstances, and their respective prospects were poor.

Policy attitudes differed on the whole along predictable lines, but in 1988 the differences between all the parties were not large. In 1991, differences over specific issues had arisen between the PRI on the one hand and the PRD and the minor parties on the other, while support for the PAN remained unrelated to specific issues.

Turning to political and institutional questions (Table 4), there is a strong association between presidential approval rating and party support. In both years, those who approve of the president's performance support the PRI; support for the opposition increases as presidential approval ratings drop. Nonetheless, in 1991, presidential approval was only moderately related to support for the minor parties.

Individual loyalty to party is strong. In 1988, the PRI retained three-quarters of its previous supporters, a figure that rose to nine-tenths in 1991. In 1988, the PAN had kept 60% of its previous supporters, a figure that reached nearly three-quarters in 1991. In 1988, the Cardenistas drew from the PRI but also substantially from the PAN and those not previously mobilized (e.g., eligible voters who chose not to vote in previous elections). Nevertheless, Cárdenas' relative success did not come about from a significant enlargement of the electoral space: the PRI still got 51% of those who had never been previously mobilized, while Cárdenas got only 28% of those voters. The Cardenista organization, moreover, had little staying power. In 1991, over a fifth of the 1988 Cardenistas returned to the PRI while nearly equal numbers split between the PRD and the minor parties (which explains their similar voter profiles).

In 1988 and 1991, PRI supporters were found especially among those who believed that the PRI would get stronger over time and that the economy would worsen and social unrest increase if some other party were to gain power. Fully 87% of the 1991 respondents who believed that the national economy would suffer under an opposition party voted for the PRI. Supporters of the opposition were much more likely to be found among those who held the opposite views. On this score, there was little difference among the opposition parties. These are among the strongest relationships we found.

We conclude, preliminarily, that attitudes toward the ruling party's future strongly shape voter choice. There was also stronger opposition in Mexico City. There was, however, a relatively weak connection between party choice and attitudes toward issues; general judgments about present and prospective economic performance were important in 1988 though they were not nearly as effective discriminators of voter choice as the question that connected the economy's future with the prospects that a party other than the PRI would gain power. Individual party loyalty was strong in the PAN, as expected, but even more so in the PRI. Attitudes toward presidential performance shaped party choice. The PAN was decidedly not a confessional party. Given the close parallel between the voter profiles for the PRD and the minor parties in 1991, the search for explanations about the Left's fragmentation should focus on factional struggles within the Left's parties and not in the electorate: the 1988 Cardenista voters did not go away in 1991, but they split their vote.

ANALYZING AND EXPLAINING PARTISAN CLEAVAGES

We turn now to explain the reasons for the vote for Mexico's opposition parties. Because we are interested in modeling discrete choices among a set of partisan alternatives, a multinomial logit specification for regression analysis is appropriate. Table 5 presents the results of this analysis, where the relative importance of demographic, economic expectation, issue attitude, and political and institutional factors can be judged.[6]

For both 1988 and 1991, we seek to explain the vote for the PAN. For 1988, we seek to explain the vote for the Cardenista coalition present on the ballot through four political parties: the Mexican Socialist party (PMS) included the old Communist party; the People's Socialist party (PPS), the Authentic Party of the Mexican Revolution (PARM), and the Frente Cardenista National Renovation party (PFCRN). The last, previously known as the Socialist Workers' party, had long been in coalition with the PRI but this time bolted to the opposition. For 1991, Cárdenas founded a new party, the PRD, into which the PMS dissolved. In 1991, the PARM, the PPS, and the PFCRN competed on their own and are listed in the column for minor parties when, together, they got 8.4% of the vote. Also included among the minor parties in 1991 are the Mexican Ecologist party, the Workers' party, the Mexican Democratic party, and the Revolutionary Workers' party; in 1991, the vote for these four parties added up to 4.3%.

The Parties and the Presidency

The only variables that identify statistically significant relationships to explain voting for all opposition parties in both 1988 and 1991 are those that refer to the parties and the presidency. In both years and for all parties,

1. the greater the belief that the PRI would get stronger, the less the likelihood of voting for an opposition party;
2. the greater the belief that the economy would improve if a party other than the PRI would gain power, the greater the likelihood of voting for an opposition party;
3. the greater the approval for the incumbent president (Miguel de la Madrid or Carlos Salinas), the less the support for the opposition;
4. the more likely a voter was to have voted PRI in a prior presidential election, the less likely this voter was to support any opposition party in either 1988 or 1991; and
5. the more likely a voter was to have voted for an opposition party in a previous election, the more likely this voter was to support an opposition party in 1988 or 1991.

A core political cleavage existed across elections between the PRI and all opposition parties. The question of the ruling party's future was the central decision facing each voter.[7] First and foremost, the voter asked, Am I for or against the PRI and the president? Consistent with this argument, for the most part these Mexican elections were not about issues. There was no statistically significant relationship between attitudes on the issues on the one hand and voting for the PAN in either year or for the minor parties in 1991 on the other. There was a modest association between voting Cardenista in 1988 and favoring the state's retention of its enterprises and between voting Cardenista in 1991 and improving the quality of the environment. Also consistent with the argument that issues mattered little, political interest was the only political variable that was never statistically significant.

Voter views about the current state of the nation's economy had no direct impact on voter choice in either year. Voter views about their current personal finances had no direct impact on voter choice in 1988 nor on voting for the Cardenistas and the minor parties in 1991. In 1991, however, those who thought that their personal finances were not in good shape

TABLE 5

Predicting Support for Mexican Opposition Parties

INDEPENDENT VARIABLES	1988[a]				1991[b]					
	PAN		CARD.		PAN		PRD		MINOR	
	β	SE_β	β	SE_β	β	SE_β	β	SE_β	β	SE_β
Female	−.16	(.20)	−.21	(.19)	−.22	(.20)	−.52	(.25)*	−.47	(.23)*
North	.38	(.26)	−.87	(.25)**	1.12	(.26)**	.48	(.35)	−.03	(.38)
South	−.29	(.28)	−.46	(.24)	−.13	(.30)	−.20	(.36)	.77	(.30)*
Federal District	.44	(.25)	.04	(.22)	.91	(.25)**	1.04	(.30)**	1.11	(.29)**
Age	−.12	(.09)	−.04	(.08)	−.12	(.09)	−.03	(.10)	−.04	(.10)
Union member	−.66	(.23)**	−.14	(.20)	−.02	(.23)	.08	(.27)	−.29	(.28)
Professional class	−.43	(.30)	−.68	(.28)**	−.55	(.32)	−.23	(.37)	−.48	(.37)
Working class	−.33	(.29)	.09	(.25)	.31	(.25)	.18	(.29)	−.12	(.28)
Education	.06	(.05)	.06	(.04)	.08	(.05)	−.04	(.06)	−.03	(.05)
Church attendance	−.01	(.06)	−.17	(.06)**	.02	(.06)	−.11	(.07)	−.06	(.07)
National economy										
Current	.09	(.12)	−.07	(.11)	−.13	(.15)	−.17	(.17)	−.13	(.16)
Future	−.36	(.12)**	−.08	(.11)	−.06	(.12)	−.19	(.15)	−.06	(.14)
Personal Finances										
Current	−.05	(.15)	−.15	(.13)	−.39	(.18)*	−.39	(.21)	−.31	(.20)
Future	−.18	(.09)*	−.11	(.08)	−.08	(.10)	.17	(.12)	−.04	(.11)
Foreign investment	.10	(.10)	.04	(.09)	.04	(.12)	−.15	(.14)	−.02	(.13)
Limit imports	−.09	(.09)	.03	(.08)	−.02	(.11)	−.05	(.13)	.12	(.12)
Pay foreign debts	.03	(.10)	−.14	(.09)	—		—		—	
State industries	−.12	(.09)	.18	(.09)*	—		—		—	
Environmentalism	—		—		.09	(.13)	.31	(.16)*	−.05	(.14)
Political interest	.01	(.09)	−.01	(.08)	−.02	(.10)	.11	(.11)	−.19	(.11)
Previous PRI supporter	−.93	(.25)**	−1.20	(.22)**	−2.04	(.29)**	−2.14	(.35)**	−2.23	(.31)**
Previous PAN supporter	2.54	(.33)**	1.12	(.33)**	2.66	(.32)**	.63	(.45)	.31	(.44)
Previous Cardenista					−.38	(.47)	1.62	(.36)**	1.28	(.34)**
Not previously mobilized	−.02	(.30)	−.14	(.27)	−.18	(.25)	−.59	(.32)	−.88	(.29)**
PRI getting stronger	−.52	(.11)**	−.52	(.10)**	−.52	(.11)**	−.53	(.14)**	−.61	(.13)**
Economy—other party	.97	(.13)**	1.20	(.12)**	.89	(.13)**	.71	(.16)**	.48	(.15)**
Unrest—other party	.30	(.18)	.30	(.16)*	.11	(.10)	.19	(.11)	.26	(.11)**
Presidential approval	−.16	(.04)**	−.15	(.04)**	−.12	(.06)*	−.23	(.06)**	−.14	(.06)*
Constant Term	.05	(.95)	.27	(.87)	−.20	(1.09)	.14	(1.33)	1.27	(1.23)

Source: Gallup interviews conducted in May 1988 and July 1991.
Note: Coefficients are multinomial logit estimates derived via maximum likelihood. Voting for the PRI served as the base category.
[a] N = 1,426, 70.1% correctly predicted.
[b] N = 1,766, 76.8% correctly predicted.
* $p < .05$.
** $p < .01$.

were more likely to vote for the PAN. On balance, these findings are inconsistent with a mode of analysis that emphasizes that citizens vote "retrospectively," that is, to reward or punish incumbent performance (Fiorina 1981).[8]

Voter views about the future of the nation's economy or about the future of their own finances had impact neither on the 1991 election nor, perhaps surprisingly, on voting Cardenista in 1988. In 1988, however, there was a strong negative association between expectations about the future of the nation's economy and voting for the PAN and a weaker negative association between expectations about the future of personal finances and voting for the PAN; that is, those who thought that the economy and their finances would get better were less likely to vote PAN. These results suggest that Mexico's 1988 presidential election had some similarities with many European elections in which voter behavior can be forecast from expectations about the economic future. Nonetheless, this pattern did not recur in 1991. Even in 1988, expectations about the nation's economic future did not explain the Cardenista vote.

Let us compare the results on two variables: general expectations about the nation's economic future and expectations about the nation's economic future if a party other than the PRI were to gain power. The latter was a powerful and systematic predictor for all parties in both elections. The former was statistically significant only with regard to PAN voting in 1988. In 1988, the coefficient for the variable concerning the economy's future if a party other than the PRI were to gain power was more than twice that for the variable measuring general expectations about the economy's future.

We conclude, therefore, that Mexicans for the most part voted neither in terms of general retrospective or general prospective assessments about the economy or their finances nor on specific economic issues. Instead, they focused on a particular kind of prospective judgment—the connection between the future of the party of the state and the impact of its fate on the economy.[9]

Political Cleavages

This argument about the preeminence of ruling-party factors in Mexican elections must be tempered, however. There are some differences among the opposition parties. In 1988, these differences hinted at the development of distinct political cleavages. Consider the PAN. Union members were much less likely to support the PAN. Except for this feature, the most striking finding about the PAN in 1988 was its emergence as a national party; the PAN was not significantly stronger or weaker in any one of the nation's regions, nor was it more or less likely to draw support on the basis of church attendance. In contrast, Cardenistas were significantly less likely to attend church. As behooves a new party, Cardenistas were weaker in some regions (the North). Cardenistas were less likely to draw support from professionals. Cardenistas were more likely to believe that social peace would not be endangered if a party other than the PRI were to win, and they were also more likely to support retaining state enterprises. This was, perhaps, a socialist party in the making.

The 1991 election picture was rather different, however. The PAN's aspirations to become a national party had suffered a setback; the PAN again became mainly a regional party, strong in the North and in Mexico City. PAN voters were more likely to think that they were not financially well off, a view at odds with the party's stereotype. The PRD shrank regionally, to derive strength mainly in the Federal District. It was also less likely to retain support from women, and its supporters tended to give higher priority to environmental issues. The minor parties were less likely to have support from women; they were also regional parties, stronger in the South and (especially) in the Federal District. We do not minimize the importance of these regional cleavages but note that they were unrelated to other demographic or issue factors. The weakening of factors other than region accentuated the centrality of ruling-party questions in the 1991 election compared to that of 1988.

Organizational Factors

There was also an important organizational story. The PRI was, indeed, a party with members loyal to it in election after election. The statistical results are very strongly significant. So, too, was the PAN, which was significantly very likely to draw from its same pool of voters in successive elections. But the PAN voter believed in occasional political adultery; in 1988, the Cardenista coalition also drew support from voters who had previously supported the PAN.

Many past PAN voters were sophisticated strategic voters. Prior to 1988, many PAN voters were motivated by their opposition to the ruling party but not by the PAN's views on particular issues. They voted for the PAN because that was the only way to oppose the PRI. Once a viable Cardenista option appeared in 1988, those who voted for the PAN in 1982 flocked to Cárdenas. In a more ideologically competitive election focused no longer on the fate of one party but on the ideas of various parties, the issues, economic expectations, social ties, and candidates, this massive PAN-ista vote switching would be less likely to happen.

The Cardenista coalition's organizational skills were insufficiently developed, however. Although many Cardenista leaders had come from the PRI, voters who had voted PRI in previous presidential elections were significantly likely *not* to vote Cardenista. (PRI voters were more loyal to their party than comparable past PAN voters.) Moreover, the Cardenistas did a poor job at mobilizing voters who had been eligible to participate in the 1982 presidential election but had chosen not to do so: the coefficient linking these previously inactive citizens to the new partisan insurgency is clearly insignificant.

In 1991, the organizational story was also noteworthy. The PRI and the PAN each drew significantly from those who had voted for it in 1988. The 1988 Cardenista vote split between the PRD and the minor parties: both drew significantly from the 1988 Cardenista coalition. Neither was able to raid the PAN much, and they were again significantly unable to raid the PRI. The minor parties were also significantly unable to mobilize support among those who had been able to vote in 1988 but had chosen not to do so.

The organizational problems on Mexico's political Left explain its disarray. The voter profile was quite similar for the PRD and the minor parties; this voter profile made them different from the PRI and the PAN. The divisions within the Left, therefore, are not explained by different voter profiles for different left-wing parties. The Cardenista coalition got the PAN's strategic voters in 1988, but none of the coalition's splinters got significant numbers of votes from the PAN in 1991. The parties of the Left were equally unable in both elections to raid steady PRI supporters, despite the PRI origins of many Cardenista leaders and the long-lasting alliances between the PRI and several of the small parties. In each election, "new" parties (the Cardenistas in 1988 and the minor parties in 1991) could not mobilize previously uncommitted voters effectively. Instead, the parties of the Left in 1991 competed for each other's voters. This is a recipe for losing elections.

To underline the PRD's problems in 1991 but also to call attention to the fragile nature of the vote for the minor parties, we turn to Table 6. Based on a huge exit poll conducted by Gallup on election day in August 1991,[10] Table 6 presents information on the timing of the voting decision. It shows the PRI's impressive strength, deriving a majority of its votes from citizens who had made a standing decision to support the party even before the names of candidates were announced. Over a third of all voters for

TABLE 6

The Timing of Vote Decisions (%)

"WHEN DID YOU DECIDE TO CAST A VOTE FOR YOUR PREFERRED PARTY?"	PRI	PAN	PRD	MINOR
Yesterday or today	17	17	21	23
Within the last month	13	15	17	19
After candidates were announced	15	24	25	21
Relied on a standing decision	56	44	37	38
Number of cases	15,778	4,282	1,516	3,772

Source: Gallup exit polls conducted in August 1991.

all parties had made a standing decision to support the PRI even before candidates were announced. To overcome the PRI, the opposition needed the loyalty of its members and the organizational capacity to mobilize new voters. The PAN exhibited much loyalty from its voters, though more conditioned on the quality of its candidates. On the other hand, nearly 4 in 10 of the PRD voters decided quite late in the campaign. This was even more the case for the minor parties. Over a fifth of those who voted for the PRD or the minor parties made up their minds within the 24 hours prior to voting. The parties of the Left lacked strong loyalties; we have previously seen that they did not mobilize the previously unmobilized. This is a weak basis to build party strength and it helps to explain the Left's election defeats.

A TWO-STEP "MODEL" FOR MEXICAN VOTING BEHAVIOR

The results from Table 5 suggest that the Mexican voter approached the elections by focusing, above all, on the fate of the PRI and the presidency, which were intimately interconnected. We believe, however, that a more ideologically competitive election lurked just behind this behavior. That is, once Mexicans got past their judgment on the ruling party, demographic and issue factors played an important role in shaping their voting decisions, and these factors divided the opposition parties between themselves. To clarify these relationships, we report at Table 7 on the hypothetical probabilities of voting for the opposition parties. These probabilities are derived from our regression analysis in Table 5. Our purpose is to show the interplay of perceptions of PRI strength and ideological predispositions.

At the top of Table 7, the expected voter choices have been computed for two types of populations. Row 1 reports on those who believed that the PRI was becoming weaker and that opposition parties could do a good job at getting the economy to grow and maintaining social peace. Row 2 reports on those who believed that the PRI was becoming stronger and that the opposition parties could not do a good job in getting the economy to grow and in maintaining social peace. In 1988, among those who thought that the PRI was becoming stronger and that the economy

TABLE 7

Expected Probabilities of Voting for an Opposition Party

VOTER PERCEPTIONS AND PREDISPOSITIONS	1988		1991		
	PAN	CARD.	PAN	PRD	MINOR
PRI becoming weaker	.31	.46	.26	.13	.16
PRI becoming stronger	.05	.05	.03	.02	.02
Voters with right-wing predispositions					
PRI becoming weaker	.47	.22	.19	.05	.12
PRI becoming stronger	.05	.02	.01	.01	.01
Voters with left-wing predispositions					
PRI becoming weaker	.08	.75	.27	.22	.14
PRI becoming stronger	.02	.09	.03	.03	.02

Source: Gallup interviews conducted in May 1988 and July 1991.
Note: These probabilities were derived from the estimated logit coefficients in Table 5 (N = 1,426 in 1988 and 1,766 in 1991). A voter's perception of the PRI's future viability is defined by three items: the expected strength of the PRI in the next 10 years, whether the national economy would prosper under an opposition party, and whether there would be increased social unrest if another party took control of the Mexican government. Left-wing or right-wing predispositions are indicated by four factors: socioeconomic status (professional versus working-class), religiosity (weekly vs. occasional church attendance), union membership, and national policy preferences. All other independent variables in Table 5 have been set to their mean values.

and social peace would suffer if a party other than the PRI were to gain power, the probability of voting PAN or Cardenista was only 5% each while the probability of voting PRI was 90%. Among those who thought that the PRI was becoming weaker and that the economy and social peace would not suffer if an opposition party were to gain power, the probability of voting PAN rose to 31%, the probability of voting Cardenista jumped to 46%, and the probability of voting PRI dropped to 23%; among these voters, Cárdenas would have been elected president. The same relationships are evident for 1991. In 1991, the PAN was still able to raise its share of vote from 3% to 26%; had they been united, the PRD and the minor parties together held 29%. In 1991, however, the PRI would win, getting 45% even among those who thought that it was becoming weaker and that the economy and social peace would not suffer if an opposition party were to gain power.

We modeled, then, two kinds of voters. One we call a left-wing voter. Such a person would come from the working class and belong to a union; attend church only occasionally; and oppose foreign investment, freer trade, continued payment of the foreign debt, and privatization of state enterprises. The other we call a right-wing voter. Such a person would come from the professional class and not belong to a union, attend church once a week, and favor the four policies opposed by the left-wing voter. (In 1991, the construction of the two hypothetical voters is the same, minus the items on the foreign debt and state enterprises, on which we have no data.)

Now consider Table 7, rows 3–6. Right-wing and left-wing voters who believed that the PRI was getting stronger and that the economy and social peace would be hurt if a party other than the PRI were to gain power were quite unlikely to vote for any opposition party in either election. They remained faithful to the PRI. What happens, however, if voters are ready to be governed by a party other than the PRI?

In 1988, among right-wing voters who believed that the PRI was getting weaker and that the economy and social peace would not suffer if an opposition party were to gain power, the probability of voting for the PAN rose to 47% while the probability of voting for Cárdenas dropped to 22%. Among left-wing voters who believed that the PRI was getting weaker and that the economy and social peace would not suffer if an opposition party were to gain power, the probability of voting for the PAN dropped to 8% while the probability of voting for Cárdenas rose to a whopping 75%. Among voters ready to live without the PRI, therefore, the PAN benefited substantially among right-wing voters and lost considerably among left-wing voters. Cárdenas benefited enormously among left-wing voters but lost more than half his vote among right-wing voters. Nonetheless, Cárdenas' support among right-wing voters was more than twice as high as the PAN's support among left-wing voters; this suggests strategic voting by right-wing voters ready to support Cárdenas because he was more likely to beat Salinas.

In 1991, among voters not scared of the PRI's defeat, the PAN actually lost support among right-wing voters but held its own among left-wing voters. The minor parties also lost support among left-wing voters and lost a bit more among right-wing voters. The PRD gained substantially among left-wing voters and lost substantially among right-wing voters, but these swings are a pale version of the 1988 Cardenista performance: in 1988, among left-wing voters who thought that the PRI was getting weaker, the probability of voting for Cárdenas was 75%, whereas in 1991, the probability of such voters voting PRD had dropped to 22%. To put it differently, in 1991 only the PRD voters responded, albeit modestly, to issue and social cleavage motivations as had been more generally the case in 1988. (The voters for the minor parties, as we have seen, were late-deciders.) Some left-wing voters were the strategic voters in 1991 when, ready for the PRI's defeat, the probability that left-wingers would vote for the PAN was 27%, while the probability that right-wingers would vote for the combination PRD–minor parties was only 17%. This strategic voting behavior explains why the PAN performed better in 1991 among all voters than among right-wing voters: in 1991, for strategic left-wing voters, the PAN was the most likely to beat the PRI.

In both elections, among voters ready to replace the party in power, issues and demographic cleavages mattered. In 1988 especially, such voters were more likely to split between the opposition parties in ways that conform to the evolution of a political party and cleavage system. Even so, we are impressed by the strategic voting behavior of an important minority of Mexico's electorate. Some right-wing and left-wing voters made electoral judgments that sought to maximize the PRI's defeat—thereby reinserting traits that focused on the fate of the ruling party (especially in 1991) even after we attempted to dampen their impact.

CONCLUSION

After decades of single-party rule and well-founded suspicions about the conduct of their rulers and the fairness of their elections, in the late 1980s and early 1990s Mexican citizens approached elections by focusing on the fate of the party that had long governed them. First and foremost, they asked themselves, Am I for or against the party of the state and its leader? The most statistically significant and consistent explanations for voting behavior in two national elections were related to these concerns: What would happen to the economy if a party other than the PRI were to win? Was the PRI getting stronger or weaker? How might one judge the president's performance? What were past patterns of partisan choice? Supporters of the PRI strongly believed in the continued viability of the party's rule, and this belief shaped their voting behavior.

Most Mexican politicians and journalists—and many scholars—interpreted these Mexican elections (especially the 1988 election) as an ideological struggle with significant social class, regional, and religious dimensions. Our conclusions differ markedly. Consistent with an explanation of voter behavior focused on the ruling party's fate, attitudes on issues hardly mattered; nor did social cleavages matter in a consistent fashion from election to election.[11] In addition, general expectations about the economy's future were important only in shaping the vote with regard to the PAN in 1988, but they did not affect voting for the PAN in 1991 nor for the Left in either election. Prospective economic expectations became significant in the analysis mainly when the question explicitly connected the economy's future to the fate of the PRI.

A related implication is that no newly politicized bloc of voters had entered Mexico's "political space." In 1988 and 1991, the principal effect of the entry of Cardenismo was to reshuffle the voters among existing parties, not to mobilize the previously unmobilized. In 1991, the parties of the Left fought each other but failed to mobilize new voters. The 1991 vote for the minor parties was more spontaneous than focused. Contrary to the views of many observers of Mexico, especially in 1988, highly charged issue-oriented conflicts, or voting behavior based on general economic expectations (as opposed to economic expectations tied to the ruling party's fate), had not replaced the old basis for division, namely, whether to retain or reject the PRI. Mexican politics changed slowly because its voters had changed little in their views.

In reaching such conclusions, we have also shed light on three matters important to Mexicanists: (1) the PRI had indeed retained the loyalty of its members despite its peculiar internal structure, (2) the PAN was not a confessional party, and (3) a large pool of potential left-wing voters awaited the organizational reconstruction of Cardenismo given the nearly identical 1991 voter profiles of the parties that in 1988 belonged to the Cardenista coalition.

There was an ideologically competitive election lurking just beneath this focus on the fate of the ruling party. To find it, however, required a heuristic exercise to identify these voters and model their hypothetical behavior. We developed a two-step "model" to suggest the typical behavior of some Mexican voters. These voters decided, first, their view on the ruling party. Many voters stopped there. The voting behavior of those open to the possibility of being governed by another party, however, resembled (especially in 1988) what might be expected in an ideologically competitive election. Issues mattered. Social cleavages mattered. There were important differences between the PAN and the Cardenistas in 1988, while the PRD's distinctive traits as an issue-and-social-cleavage-oriented opposition party came through in 1991. There is evidence, moreover, of substantial strategic voting behavior among both right-wing and left-wing voters depending on the specific context of each election.

For the PRI, these findings suggest the importance of partisanship and presidential performance as well as its need to cast doubt on any opposition party's ability to govern. For the parties of the Left, these findings point to the imperative need to rebuild a united coalition. For the PAN, these findings indicate that it should position itself as an alternative ruling party, taking advantage of its victories in some gubernatorial elections in the early 1990s; once Mexican voters believe that a party other than the PRI can govern, the PAN does well among right-wing voters and can draw on left-wing strategic voters.

For Mexico, the shaping of its political arena is under way. Mexicans differed on issues; the various parties could express various social cleavages. But the full flowering of these issue and social cleavages in the political arena awaited further democratization. The preeminent concern over the fate of the ruling party in Mexican elections still prevented the full emergence of the representative and contestatory roles that parties perform in ordinary elections in other countries. Mexican democracy was still slouching to be born, and the issues that would one day enliven its politics remained hidden until that Bethlehem would be reached.

APPENDIX: THE VARIABLES

For the purpose of our analysis, we have focused on the independent variables measured by the following questions, which are listed in the sequence in which they appear in Tables 2–4, the same sequence being repeated in Table 5.

Demographic Questions (Tables 2 and 5)

Male/Female
From Mexico's North
From Mexico's Center (excluding Mexico City)
From Mexico's South
From the Federal District (i.e., Mexico City)
Age
Union Member: Yes/No

Class, as measured by the respondent's profession: "Please tell me which of these kinds of work best describes the work that you do." A card is then shown listing many different kinds of work. *Professional* includes lawyer; medical doctor; professor; engineer; nurse; public accountant; computer programmer; systems analyst; musician; government or state enterprise director, manager or official; and owner or manager of a factory, store, or other private firm. *Middle* includes white-collar positions such as secretary, postal worker, bank clerk, sales clerk, vendor, salesman, police officer, fire fighter, restaurant personnel, pharmacist, hairdresser, and the like. *Working* includes skilled-trades such as mechanic, plumber, electrician, and painters, as well as factory worker (at various levels of skill), cab driver, chauffeur, machine operator, lumberman, farmer or farm worker, and fisherman.

Education: "What was your last year in school?" We coded as *primary* those who have at most completed primary education; *secondary*, those who have begun secondary schooling or, at most, completed it; *preparatory*, those who have begun the upper end of university-oriented high school education or, at most, completed it; *university*, those who have some university education or who may have completed it.

Church attendance: "With what frequency do you attend church?"

More than once a week, once a week, two or three times per month, every month, from time to time, or never? *Weekly* refers to those who attend church at least once a week, *occasionally*, those who attend church at least "every once in a while" but no more than two or three times per month. *Never* means just that.

Issue Questions (Tables 3 and 5)

"How would you assess the current condition of the nation's economy? Would you say that the nation's economy is doing very well, well, badly, or very badly?" We have collapsed the four-point response into *good/bad*. *1988 Version*. "Would you say that at the end of the forthcoming six-year presidential term Mexico's economic situation would be better than it is now, would be the same, or would be worse?" We have used only the *better/worse* responses. *1991 Version* "Would you say that in three years Mexico's economic situation would be better than it is now, would be the same, or would be worse?" We have used only the *better/worse* responses.

"How would you assess your own personal economic situation? Would you say that your personal economic situation is very good, good, bad, or very bad?" We have collapsed the four-point response into *good/bad*. *1988 Version*. "How do you expect that your own economic situation will be within a year—much better, a bit better, a bit worse, or much worse than it is now?" We have collapsed the four-point response into *better/worse* and ignored those respondents who volunteered "the same." *1991 Version*. "How do you expect that your own economic situation will be within three years—much better, a bit better, a bit worse, or much worse than it is now?" We have collapsed the four-point response into *better/worse* and ignored those respondents who volunteered "the same."

"Considering all the consequences, good and bad, in your view is investment in Mexico by foreign companies positive or negative for the country?" *1988 Version*. "With regard to imports of foreign products into Mexico, should the next government facilitate the import of those goods or should it limit their importation?" *1991 Version*. "With regard to imports of foreign products into Mexico, in your view is the policy of facilitating the import of those goods basically a good policy or is it basically a bad policy for the people of our country?"

Asked Only in 1988. "In your view, should the next government continue to pay the country's foreign debt or should it stop payment?"

Asked Only in 1988. "With regard to state enterprises, what do you think that the next government should do: keep most of them or sell most of them to private enterprises?"

Asked Only in 1991. "Thinking [of] all the areas in which the government has to invest its effort and money—such as education, road building, agricultural output, etc.—how much importance should be given to the protection and improvement of the environment? Would you say that improving the quality of air and water should be the government's most important priority, should be an important but not the most important priority, should have a moderate level of importance, or should have no importance at all?" For our analysis, the two middle answers have been combined as *moderate importance*.

Political Questions (Tables 4 and 5)

1988 Version. "Using a scale from 1 to 10, how would you rate President Miguel de la Madrid's job as president?" *1991 Version*. "Using a scale from 1 to 10, please rate on this paper, without showing it to me, the job performance of Carlos Salinas de Gortari as President of the Republic." (Then a "secret ballot box" was used.) In both years, respondents were told that 1 is least favorable and 10 is most favorable. For both years, we coded ratings of 9 or 10 as *very high*, 7 or 8 as *favorable*, 4–6 as *mediocre*, and 3 or below as *poor*.

"Referring to the card I'm showing you, please tell me how much interest in politics would you say that you have? None, little, some, much?"

In both 1988 and 1991, the same procedure was followed to ask about past voting behavior: "Did you vote in the previous presidential elections?" Those who said *yes* were then asked, "For which candidate did you vote?" Voters then were given the "secret ballot box" to answer this question. We recoded their answers into the party affiliations of the candidates. *Not previously mobilized* refers only to those respondents who were eligible to vote in the previous presidential election but chose not to do so.

"Ten years from now, do you expect that the PRI will be a stronger party than it is today, a weaker party that it is today, or about as strong as it is today?"

"If a party other than the PRI were to gain power, do you think that Mexico's economic conditions would improve, remain the same, or worsen?"

"If a party other than the PRI were to gain power, do you think that there would be problems for the country's social peace or that there would not be problems with social peace?"

Notes

1. We are very grateful to Jeffry Frieden, Jennifer Widner, and Ashutosh Varshney for many thoughtful and constructive comments.

2. This section draws in part from Domínguez and McCann 1992.

3. Most public opinion polls in Mexico are sponsored by a political party and are of dubious quality. Ours, on the other hand, were financed by independent media organizations; they are the best available to study Mexican political behavior. The 1988 poll was commissioned by ECO, Inc., a Los Angeles television station that is a subsidiary of Mexico's large private television network, TELEVISA. The second poll was financed directly by TELEVISA. One of us (Domínguez) worked as a consultant for both polls. We are grateful to Richard W. Burkholder, vice president, Gallup Organization, and Ian M. Reider, president, IMOP/Gallup México, for allowing us to use the data for this research.

4. Originally 3,000 interviews were planned. Gallup México discarded 40 of those interviews because they were unreliable.

5. Almost half of those who said through the "secret ballot" that they would vote for Cárdenas or for the PAN said something different when they were asked a similar question openly.

6. The functional form of our logit equations may be written as follows. If we code a preference for the PRI in 1988 as 1, PAN support as 2, and Cardenista as 3, the probability of an individual voting for each of the parties is

$$Prob(Vote\ PAN) = \exp(x\beta_1)/[1 + \exp(x\beta_1) + \exp(x\beta_2)]$$

$$Prob(Vote\ CARD) = \exp(x\beta_2)/[1 + \exp(x\beta_1) + \exp(x\beta_2)]$$

$$Prob(Vote\ PRI) = 1/[1 + \exp(x\beta_1) + \exp(x\beta_2)].$$

In these equations, exp refers to the exponential function, x stands for a vector of all independent variables, and β_1 and β_2 correspond to the logit regression coefficients computed via maximum likelihood for the two equations in 1988. For 1991, the dependent variable has four categories rather than three, with the minor parties taking on a value of 4. The functional form in this case is a simple extension of these specifications. Included in each denominator are the coefficients from the additional equation for the minor parties category. For example,

$$Prob(Vote\ PAN\ 1991) = \exp(x\beta_1)/[1 + \exp(x\beta_1) + \exp(x\beta_2) + \exp(x\beta_3)],$$

where the β_3 values are taken from the coefficients listed on the far right-hand column of Table 5. By coding the dependent variable in this way, the probability of voting for the PRI serves as our base alternative.

7. The 1988 poll asked questions that permit the construction of an index of personality trait ratings for each of the presidential candidates. Inclusion of such an index would not change these substantive conclusions. To be sure, a favorable impression of the three presidential candidates in 1988 influenced voting choice quite significantly, but the independent variables reported in Table 5 remain significant (with the exception of the item on state enterprises). The effect of including an index of candidate ratings would reinforce the finding that explicitly political factors explain voting choices.

8. It is possible, of course, that economic evaluations indirectly influenced vote choice through perceptions of the PRI's long-term viability and its future governing capabilities. To illustrate, consider the following multinomial logit coefficients (with standard errors in parentheses) based on the 1988 sample. The estimates in columns 1 and 2 are taken directly from Table 5, while those in columns 3 and 4 come from an equation that did not include the three predictors related to the PRI as an institution (*PRI getting stronger, Economy—other party,* and *unrest—other party*).

National Economy	Including "Institutional"		Excluding "Institutional"	
	PAN	Cardenista	PAN	Cardenista
Current	.09 (.12)	−.07 (.11)	−.11 (.14)	−.22 (.11)*
Future	−.36 (.12)**	−.08 (.11)	−.39 (.14)**	−.09 (.11)

*$p < .05$.
**$p < .01$.

When the three prospective judgments related to the PRI are left out of the equations, evaluations of Mexico's economic performance in 1988 are more closely connected to voting choice. This suggests that attitudes toward the economy have some bearing on presidential preferences but that such effects are largely mediated by the more powerful items that tap into the PRI's future viability and governing capabilities.

9. Of course, it is possible that judgments regarding the PRI's viability and capabilities were inferred or rationalized on the basis of the respondent's vote choice. Given all presently existing data sets, it remains impossible to model this potential dynamic thoroughly. Nevertheless, our findings from a series of regressions estimated via instrumental variables (available from the authors upon request) suggest that prospective evaluations of the PRI did, indeed, cause voters to support or oppose the party.

10. We use the exit poll only for Table 6. The exit poll relied on a multistage-area-probability sample, with each geographic entity having the same probability of falling in the sample according to its weight in the electoral registry. There were 350 sampling sites throughout the country; the statistical margin of error was ±1%.

11. That issues have little impact on the voting choice is not surprising from a comparative perspective; that demographic factors matter so little is more surprising, both in general and for Mexicanists. For comparative findings, see Harrop and Miller 1987, chaps. 6–7.

References

Almond, Gabriel, and Sidney Verba. 1965. *The Civic Culture.* Boston: Little, Brown.

Bailey, John J. 1988. *Governing Mexico: The Statecraft of Crisis Management.* New York: St. Martin's.

Basáñez, Miguel. 1982. *La lucha por la hegemonía en México.* Mexico: Siglo XXI.

Basáñez, Miguel. 1992. "Encuestas de opinión en México." In *México: auge, crisis y ajuste.* Eds. Carlos Bazdresch, Nisso Bucay, Soledad Loaeza, and Nora Lustig. Mexico: Fondo de Cultura Económica.

Butler, Edgar W., James B. Pick, and Glenda Jones. 1991. "Political Change in the Mexico Borderlands." In *Sucesión Presidencial: The 1988 Mexican Presidential Election.* eds. Edgar W. Butler and Jorge A. Bustamante. Boulder: Westview.

Chand, Vikram. 1991. "Civil Society, Institutions, and Democratization in Mexico: The Politics of the State of Chihuahua in National Perspective." Ph.D. diss., Harvard University.

Domínguez, Jorge I., and James A. McCann. 1992. "Whither the PRI? Explaining Voter Defection in the 1988 Mexican Presidential Elections." *Electoral Studies* 11:207–22.

Fiorina, Morris P. 1981. *Retrospective Voting in American Elections.* New Haven: Yale University Press.

Garrido, Luis Javier. 1987. "Un partido sin militantes." In *La vida política mexicana en la crisis,* ed. Soledad Loaeza and Rafael Segovia. Mexico: El Colegio de México.

Gómez Tagle, Silvia. 1989. "La dificultad de perder: el partido oficial en la coyuntura de 1988." *Revista mexicana de sociología* 51:239–260.

Harrop, Martin, and William L. Miller. 1987. *Elections and Voters.* London: Macmillan.

Huntington, Samuel P. 1991. *The Third Wave: Democratization in the Late Twentieth Century.* Norman: University of Oklahoma Press.

Lewis-Beck, Michael. 1988. *Economics and Elections.* Ann Arbor: University of Michigan Press.

Lipset, Seymour Martin, and Stein Rokkan. 1967. "Cleavage Structures, Party Systems, and Voter Alignments." In *Party Systems and Voter Alignments,* ed. Seymour Lipset and Stein Rokkan. New York: Free Press.

Loaeza, Soledad. 1989. *El llamado de las urnas.* Mexico: Cal y Arena.

Middlebrook, Kevin J. 1989. "Union Democratization in the Mexican Automobile Industry: A Reappraisal." *Latin American Research Review* 24:69–93.

United Nations Economic Commission for Latin America and the Caribbean. 1989. *Balance preliminar de la economía de América Latina y el Caribe, 1989.* LC/G.1586.

United Nations. 1991. *Preliminary Overview of the Economy of Latin America and the Caribbean, 1991.* LC/G.1696.

WGBH Educational Foundation. 1988. "Mexico: End of an Era, 1982–1988." Public Broadcasting System.

Jorge I. Domínguez is Professor of Government, The Center for International Affairs, Harvard University, Cambridge, MA 02138.

James A. McCann is Professor of Political Science, Purdue University, West Lafayette, IN 47907.

American Political Science Review Vol. 89, No. 1 March 1995

MODELING NEGATIVE CAMPAIGNING

STERGIOS SKAPERDAS and BERNARD GROFMAN *University of California, Irvine*

Negative campaigning is an important aspect of campaign competition but plays little or no role in existing models of campaigns. Within the context of plurality elections for a single office we model the incentives that affect the use of negative campaigning. Under simplifying but still quite general assumptions we show a number of results, including the following key conclusions: (1) for two-candidate competition the front-runner will engage in more positive and less negative campaigning than the opponent; (2) in a three-candidate contest with one candidate clearly trailing by a large margin and playing mainly a spoiler role, that candidate will only engage in positive campaigning; and (3) in any three-candidate contest, no candidate engages in negative campaigning against the weaker of his two opponents, so that to the extent there is negative campaigning, it will be directed against the front-runner or it will come from the front-runner. These results have direct empirical applications to multicandidate primaries and nonpartisan contests and can provide insight into recent general elections as well.

We present a model of negative campaigning, an important aspect of campaign competition that has received a great amount of journalist attention. Negative campaigning has been a focus of political communication studies (see, e.g., Garramone 1984; Johnson-Cartee and Copeland 1989, 1991; Pfau and Kenski 1990), as well as some important experimental research (e.g., Ansolabehere and Iyengar 1993, Roddy and Garramone 1988). However, it has largely been neglected by formal modelers. (Harrington and Hess 1993 and Thomas 1990 are notable exceptions). Adapting terminology from Surlin and Gordon, we use the term *negative campaigning* to refer generally to that which "attacks the other candidate personally, the issues for which the other candidate stands, or the party of the other candidate" (1977, 93).

While there is dispute about the prevalence and intensity of negative political campaigning today as compared to some earlier periods of American history—when, according to one noted historian, "no accusation was too coarse or too vulgar to be made" (Wood 1978, 109; see also Beiler 1992)—most students of contemporary politics in the United States are in agreement that the level of negativity in campaigns has, in general, been rising considerably in recent decades. For example, Pfau and Kenski state that "the 1980s experienced an explosive growth in attack politics" (1990, 13), while Young asserts that "today one of two political ads are negative; twenty years ago only about one in five were" (1987, 66). Moreover, the consensus among political journalists and consultants, even if not backed by evidence that is fully compelling in social science terms, is that "[while] there is room for argument about whether negative ads will damage the system in the long run, there is no argument about their short-term impact, [that] they work and they win elections. Voters pay attention to them" (Ehrenhalt 1985, 2560).[2]

Ansolabehere and Iyengar note that until recently, academic research has suggested that "campaign communication in general, and advertising in particular, have little persuasive impact on voters" and go on to comment that "the argument that campaign advertising has only minimal effects is difficult to reconcile with candidate's actual behavior. All serious campaigns invest heavily in advertising" (1993, 1–2). The increasingly larger proportions of campaign budgets that are devoted to the media, survey studies that show the striking memorability of some political ads, especially "attack" ads (e.g., Johnson-Cartee and Copeland 1989), experimental work by scholars such as Ansolabehere, Iyengar, and Kinder showing the strong effects of political advertising and political rhetoric (e.g., Ansolabehere et al. 1994; Iyengar, Peters, and Kinder 1982), and the recent work of Bartels (1993) showing measurement error effects in using survey data to judge the impact of campaign events on voter choice all point toward paying far more attention to campaign-specific processes of political persuasion than has often been the case in political science models of voter choice based on party identification, issue proximity, or the impact of economic conditions on incumbent performance evaluations.

We are here interested in specifying how key factors such as the closeness of the race and the number of candidates can be expected to affect the division of each candidate's efforts between positive and negative campaigning. We model a limited but important part of the big picture of political competition that is omitted in most standard Downsian modeling, namely, the decisions by candidates to inform voters about (alleged) negative aspects of their opponent's character, history, and issue stands in addition to informing voters about themselves and their positions.

Our model builds on various commonsense attributes of present-day politics (e.g., that candidates know where they stand in the polls but do not know future vote shares with certainty, that some voters are undecided, that voters can change their mind over the course of the campaign and even move from support to a position of undecided or vice versa, and that campaigning can matter) and one key assumption about political competition, namely, that negative campaigning reduces the support level of the candidate who is attacked and may also reduce, at least in the short run, the attacking candidate's own support because of voters who are disgruntled by negative campaigning. The generally alienating effect of negative campaigning on voters has been identified in survey data (Garramone 1984) and in experiments (Ansolabehere et al. 1994). For example, Garramone reports that over three-quarters of the survey respondents expressed disapproval in their evaluation of negative advertising. As for the losses accruing to the attacker, Garramone (1984) has found strong support and refers to the effect as the "boomerang effect."[3] To allow for a candidate rationally to engage in negative campaigning, we maintain that the negative effect (vote loss) must be greater for the candidate being attacked than for the attacker.[4]

Under a simple but still quite general formulation (e.g., positing a function that describes what happens to each candidate's expected vote share as a result of any allocation of effort to negative and positive campaigning), in which we model a campaign as a series of snapshots of an evolving dynamic process,[5] we show a number of interesting results, including the following key conclusions:

Ceteris paribus, (1) for two-candidate competition there will often be a mix of positive and negative campaigning, but the front-runner can be expected to engage in more positive, and less negative, campaigning than his opponent; (2) in two-candidate competition, a candidate's negative campaigning is often greater the stronger the opponent's support; (3) in a three-candidate contest with one candidate clearly trailing by a large margin and playing mainly a spoiler role, that candidate will only engage in positive campaigning; and (4) in any three-candidate contest, no candidate engages in negative campaigning against the weaker of his two opponents, so that any negative campaigning will be directed against the front-runner or will come from the front-runner.

We shall consider at the end how well these conclusions fit evidence on negative campaigning. Now we turn to an exposition of the model.

TWO-CANDIDATE COMPETITION

There are two candidates, labeled 1 and 2. Each candidate has one unit of time or resource that can be allocated between positive campaigning, denoted y_i, and negative campaigning, x_i, so that

$$1 = x_i + y_i \quad i = 1, 2. \tag{1}$$

We suppose that the initial support for each candidate, r_1 for 1 and r_2 for 2, is common knowledge. The same is, therefore, true for the fraction of remaining undecided voters $R = 1 - r_1 - r_2$. Because undecided voters can be expected to be both more active seekers of information about the candidates and more liable to be convinced by one of them compared to the voters who are already decided, we suppose that positive campaigning attracts undecided voters. In particular, for any given pair of positive campaigning by the two candidates, (y_1, y_2), let $q^i(y_1, y_2)$ denote the share of undecided voters received by candidate $i (= 1, 2)$. We assume the following properties for these *positive campaigning sharing functions:*

ASSUMPTION 1. $q^1(y_1, y_2)$ *is increasing in* y_1 *and decreasing in* y_2; $q^2(y_1, y_2)$ *is decreasing in* y_1 *and increasing in* y_2.

ASSUMPTION 2. $q^1(y_1, y_2) = q^2(y_2, y_1)$ *for all* (y_1, y_2).

ASSUMPTION 3. $q^1(y_1, y_2) + q^2(y_1, y_2) = 1$ *for all* (y_1, y_2).

Assumption 1 says that the share of undecided voters received by each candidate is increasing in the amount of positive campaigning undertaken by the candidate himself and decreasing in the amount of the opponent's positive campaigning. Assumption 2 is a symmetry property. It implies that if both candidates undertake the same amount of positive campaigning, then they attract equal shares of the undecided voters. Finally, assumption 3 states that each of today's undecided voters decides to support one of the two candidates tomorrow. We adopt this property for convenience; our results would follow through with more realistic, but more complicated, alternatives.[6] Given assumptions 1–3, it can be shown that there is a symmetric function $q(\cdot, \cdot)$, increasing in its first argument, decreasing in its second argument, and such that $q^1(y_1, y_2) = q(y_1, y_2)$ and $q^2(y_1, y_2) = q(y_2, y_1) = 1 - q(y_1, y_2)$.[7] From now on, we shall use the convention of denoting $q^1(y_1, y_2) = q(y_1, y_2)$ by q and thus denoting $q^2(y_1, y_2) = 1 - q(y_1, y_2)$ by $1 - q$. A general class of functions satisfying assumptions 1–3 is

ASSUMPTION 4. $q^i(y_1, y_2) = f(y_i)/[f(y_1) + f(y_2)]$, *where* $f(\cdot)$ *is an increasing nonnegative function.*

Whereas positive campaigning attracts undecided voters in the way we have just specified, negative campaigning is undertaken in order to reduce the support of one's opponent by turning a fraction of the opponent's current supporters into undecided voters. Negative campaigning, however, is not just harmful for the recipient of the negative attack. It harms the initiator as well, through the "boomerang effect," by turning off some of the initiator's current supporters and turning them into undecided voters but at a lower rate than for the recipient of negative campaigning. As discussed in the introduction, incorporating the "boomerang effect" makes our model consistent with some strong empirical evidence. In its absence, however, our qualitative results would be unaffected, in the sense that candidates would engage in more negative campaigning compared to the case with the "boomerang effect" but the relative magnitudes and other characterization results would be unaffected. In particular, for any given pair (x_1, x_2) of negative campaign allocations by the two candidates, the reduction in support for candidates 1 and 2 are assumed to take the form

$$B(x_1 + Ax_2) \cdot r_1 \quad \text{and} \quad B(x_2 + Ax_1) \cdot r_2, \tag{2}$$

respectively, where A (>1) measures by how much more negative campaigning hurts the victim relative to the instigator of a negative attack. The parameter B is an overall measure of the effectiveness of negative campaigning; a higher B implies a higher sensitivity of voters to negative campaigning.

Recapitulating the different pieces of the model thus far, the candidates choose how much effort to allocate between positive and negative campaigning (see equation 1); the positive campaigning efforts determine the relative shares of the undecided voters received by the two candidates; and negative campaigning determines the support subtracted from each candidate according to equation 2 with the lost voters becoming new undecided voters.

Putting these pieces together, the support for each candidate in the subsequent period becomes:

$$r_1^0 = r_1 + q(y_1, y_2)R - B(1 - y_1 + A(1 - y_2))r_1 + \varepsilon_1$$

$$r_2^0 = r_2 + (1 - q(y_1, y_2))R - B(1 - y_2 + A(1 - y_1))r_2 + \varepsilon_2. \tag{3}$$

The ε_is are zero-mean error terms subject to the restriction $r_i^0 + r_j^0 \leq 1$. Note that by the use of equation 1, we have eliminated the x_is, and the new support levels in equation 3 can be considered just functions of the y_is; this makes the analysis easier later on, but remember that $1 - y_i = x_i$ for both i.

We take as each candidate's objective the maximization of the difference between expected support and the expected opponent's support (i.e., $V^i = E(r_i^0) - E(r_j^0)$, where $i \neq j$ and $E(\cdot)$ is the expectation operator) which, given equation 3, yields the following payoff functions:[8]

$$V^1(y_1, y_2) = (2q(y_1, y_2) - 1)R - B(1 - y_1 + A(1 - y_2))r_1 + B(1 - y_2 + A(1 - y_1))r_2 + r_1 - r_2$$

$$V^2(y_1, y_2) = (1 - 2q(y_1, y_2))R - B(1 - y_2 + A(1 - y_1))r_2 + B(1 - y_1 + A(1 - y_2))r_1 + r_2 - r_1. \tag{4}$$

These two payoff functions along with the set of possible positive campaigning pairs (y_1, y_2) define a game in strategic (normal) form. We shall employ the concept of noncooperative (Nash) equilibrium as a solution concept. A strategy pair (y_1^*, y_2^*) is an *equilibrium* if

$$V^1(y_1^*, y_2^*) \geq V^1(y_1, y_2^*) \quad \text{for all } y_1$$

and

$$V^2(y_1^*, y_2^*) \geq V^2(y_1^*, y_2) \quad \text{for all } y_2. \tag{5}$$

To guarantee that such an equilibrium exists we need to introduce an additional property on the sharing function $q(\cdot, \cdot)$:

ASSUMPTION 5. $q_{11} \equiv \partial^2 q(y_1, y_2)/\partial y_1^2 \leq 0$ *(which, by symmetry, is equivalent to* $q_{22} \equiv \partial^2 q(y_1, y_2)/\partial y_2^2 \geq 0$)

According to assumption 5, a candidate's share of the undecided voters is a concave function of the candidate's own positive campaigning effort. In other words, other things equal, there are diminishing returns to each candidate's positive campaigning; the more positive campaigning undertaken, the lower the extra support received. We think this property is empirically plausible, but it could prove that some campaigns do not conform to it. In such a case a pure-strategy equilibrium might not exist. A mixed-strategy equilibrium, however, would exist; and although it would be technically very difficult to characterize such an equilibrium, there is no a priori reason for a reversal of the various results we obtain later on.

THEOREM 1. *In two-candidate competition make assumptions 1–3 and 5. Then the payoff function of each candidate is concave in the candidate's own strategy, and a pure-strategy equilibrium exists.*

(For the proofs of theorem 1, as well as of other results not found in the main body of the paper, see the Appendix.)

For the remainder of this section, we assume that assumptions 1–3 and 5 are satisfied. We now proceed with the characterization of equilibrium strategies and outcomes. Let $V_1^1 \equiv \partial V^1(y_1, y_2)/\partial y_1$ and let V_2^2 be similarly defined. From equation 4 it follows that

$$V_1^1(y_1, y_2) = 2q_1(y_1, y_2)R - B(Ar_2 - r_1) \tag{6}$$

$$V_2^2(y_1, y_2) = -2q_2(y_1, y_2)R - B(Ar_1 - r_2), \tag{7}$$

(where $q_i = \partial q/\partial y_i$). The first term of each of these derivatives represents the marginal benefit of putting an infinitesimally small extra effort into positive campaigning, whereas the second term represents its marginal cost, which also represents the marginal benefit of negative campaigning. If in equilibrium a candidate were to put positive amounts of effort into both positive and negative campaigning (in symbols, $0 < y_i^* < 1$), then the marginal benefit and the marginal cost must be equated and the derivative in equations 6–7 would be set equal to zero for that candidate. (Otherwise, the candidate would have an incentive to change strategy, thus contradicting the definition of equilibrium in equation 5.) There is no reason, however, that in every possible circumstance the equilibrium must satisfy this condition and in fact it does not. Cases in which one or both candidates choose to put all their effort into either positive or negative campaigning (y_i^* is equal to 0 or 1 for at least one $i = 1, 2$) are interesting and make intuitive sense, as we shall see.

Suppose, for specificity, that $Ar_2 \leq r_1$, which means, given that $A > 1$, that the initial support for candidate 2 is lower than that for candidate 1 and, depending on how large A is, it can be much lower. Then, since by equation 6 q_1 is positive, it must be the case, for all possible values of (y_1, y_2), that

$$V_1^1(y_1, y_2) > 0 \quad (\text{where } r_1 \geq Ar_2). \tag{8}$$

Since, by theorem 1, V^1 is concave in y_1, the optimal choice of y_1 must equal one (all effort put into positive campaigning) regardless of candidate 2's choice of y_2. It then follows that in equilibrium candidate 1 would put all effort into positive campaigning. Obviously, when $r_1 \geq Ar_2$ we cannot simultaneously have $r_2 \geq Ar_1$; thus we cannot use the same argument to establish that candidate 2 puts all effort into positive campaigning whenever candidate 1 does so. In fact, an inspection of equation 7 in this case reveals that when $r_1 \geq Ar_2$, for candidate 2 to put all effort into positive campaigning becomes less likely, though not impossible. In the absence of a condition like $r_1 \geq Ar_2$, equation 8 could be true if B (the relative *effectiveness* of negative campaigning) is small enough or A (the harm of negative campaigning to the receiver relative to the harm done to the attacker) is low enough. We summarize our result thus far as a proposition:

Proposition 1. *If a candidate's initial support is sufficiently high relative to that of the opponent, that candidate will not engage in negative campaigning. If negative campaigning is sufficiently ineffective or sufficiently harmful to the attacker, negative campaigning is less likely.*

Next consider a sharing function $q(\cdot, \cdot)$ such that the derivative $q_1(0, y_2)$ (or, symmetrically, $q_2(y_1, 0)$) is finite. Then for B and A sufficiently high or r_1 sufficiently low, the following inequality may be true regardless of the value of y_2:

$$V_1^1(0, y_2) = 2q_1(0, y_2)R - B(Ar_2 - r_1) \leq 0. \tag{9}$$

In this case, candidate 1 would always choose to put all effort into negative campaigning ($y_1^* = 0$). Again, we summarize this result below.

Proposition 2. *If candidate i's initial support is sufficiently low or attacking one's opponent is sufficiently effective and does not do much harm to the attacker, then candidate i may put all of his effort into negative campaigning.*

We now move away from the polar cases we just examined and concentrate on equilibria (y_1^*, y_2^*) in which both candidates choose to do both positive and negative campaigning ($0 < y_1^* < 1$ and $0 < y_2^* < 1$). Note that for this to happen, it is necessary to have both $Ar_1 > r_2$ and $Ar_2 > r_1$. Clearly at such an equilibrium the derivatives in equations 6–7 are equal to zero, or

$$2q_1(y_1^*, y_2^*)R - B(Ar_2 - r_1) = 0$$

$$-2q_2(y_1^*, y_2^*)R - B(Ar_1 - r_2) = 0, \tag{10}$$

which imply

$$q_1(y_1^*, y_2^*) = B(Ar_2 - r_1)/(2R)$$

and

$$-q_2(y_1^*, y_2^*) = B(Ar_1 - r_2)/(2R),$$

respectively. Suppose, for specificity, that candidate 1 has more initial support than candidate 2, so that $r_1 > r_2$. This implies $Ar_1 - r_2 > Ar_2 - r_1$, which, used in equation 10, yields $q_1(y_1^*, y_2^*) < -q_2(y_1^*, y_2^*)$. By Lemma I in the Appendix (derived under assumptions 1–5) and, to guarantee strict inequalities, a minor technical condition), this inequality is equivalent to $q(y_1^*, y_2^*) > 1/2$, which is, in turn, equivalent to $y_1^* > y_2^*$. Therefore, we have shown the following result.

Proposition 3. *The front-runner (i.e., the candidate with higher initial voter support) engages in more positive and less negative campaigning than his opponent.*

To facilitate a more detailed analysis, we now examine equilibria under the following functional form of $q(\cdot, \cdot)$ (note that this function is not defined at $(y_1, y_2) = (0, 0)$):

Assumption 6. $q(y_1, y_2) = \dfrac{y_1}{y_1 + y_2}$.

Under assumption 6, the equilibrium conditions (equation 10) become

$$\frac{2y_2}{(y_1 + y_2)^2} R - B(Ar_2 - r_1) = 0 \tag{11}$$

$$\frac{2y_1}{(y_1 + y_2)^2} R - B(Ar_1 - r_2) = 0. \tag{12}$$

One way of getting a better sense of the strategic interactions is to derive the reaction functions of the two candidates using equations 11–12.[9] Denote by $\rho_1(y_2)$ the reaction function of candidate 1 and by $\rho_2(y_2)$ the reaction function of candidate 2. Then it can be shown, by using equations 11–12, that

$$\rho_1(y_2) = y_2^{1/2}\left[\frac{2R}{(Ar_2 - r_1)B}\right]^{1/2} - y_2$$

$$\rho_2(y_1) = y_1^{1/2}\left[\frac{2R}{(Ar_1 - r_2)B}\right]^{1/2} - y_1. \tag{13}$$

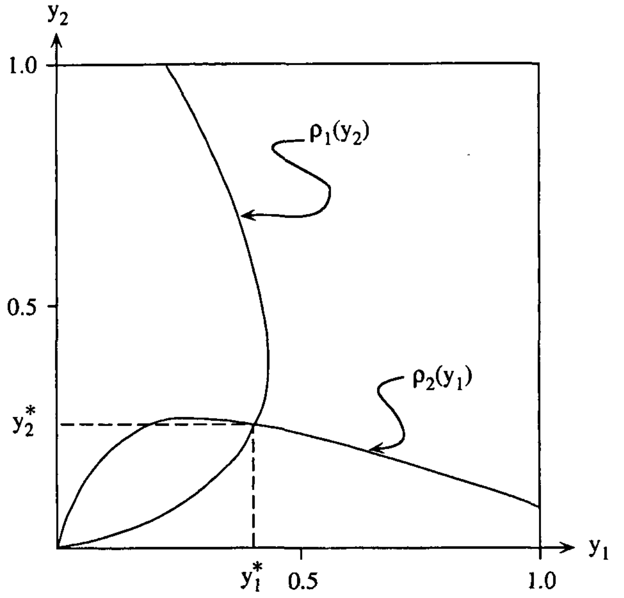
To find the equilibrium, we can set $y_1 = \rho_1(y_2)$ and $y_2 = \rho_2(y_1)$ and then solve equation 13 for y_1 and y_2. A typical diagrammatic configuration of equation 13 is shown in Figure 1. An equilibrium is at an intersection of the two graphs. Since assumption 6 is undefined at $(y_1, y_2) = (0, 0)$, the relevant intersection of ρ_1 and ρ_2 is at (y_1^*, y_2^*), as indicated in the figure.

It is straightforward, though tedious, to find a general closed-form solution for the interior equilibrium (y_1^*, y_2^*):

$$y_1^* = \frac{2R(Ar_1 - r_2)}{B(Ar_1 - r_2 + Ar_2 - r_1)^2} = \frac{2(1 - r_1 - r_2)(Ar_1 - r_2)}{B(r_1 + r_2)^2(A - 1)^2} \tag{14}$$

FIGURE 1

Reaction Functions with A = 2, B = 1, r_1 = .4, and r_2 = .35



$$y_2^* = \frac{2R(Ar_2 - r_1)}{B(Ar_1 - r_2 + Ar_2 - r_1)^2} = \frac{2(1 - r_1 - r_2)(Ar_2 - r_1)}{B(r_1 + r_2)^2(A - 1)^2} \qquad \textbf{(15)}$$

Based on equations 14–15 we have the following comparative static results:

Proposition 4. *Suppose* q(·, ·) *satisfies assumption 6. Then:*

a. *Both candidates increase their positive campaigning when the fraction of undecided voters increases* ($\partial y_i^*/\partial R >$ *for both* i = 1, 2).

b. *Both candidates reduce their positive campaigning when the overall effectiveness of negative campaigning increases* ($\partial y_i^*/\partial B < 0$ *for both* i = 1, 2).

c. *The front-runner will always reduce positive campaigning when* A *increases (i.e., when the harm to the recipient of negative attacks increases). The follower will also reduce positive campaigning when* A *increases, unless the follower is sufficiently far behind the front-runner* ($\partial y_1^*/\partial A \lesseqgtr 0$ *as*

$$\left.\frac{r_1}{r_2} \gtrless \frac{2}{A+1} \quad \textit{and} \quad \partial y_2^*/\partial A \lesseqgtr 0 \quad \textit{as} \quad \frac{r_2}{r_1} \gtrless \frac{2}{A+1}\right).$$

d. *Negative campaigning is increasing in the opponent's initial support* ($\partial y_i^*/\partial r_j < 0$, i ≠ j).

e. *The effect of a candidate's own support level on equilibrium allocation of effort between positive and negative campaigning is ambiguous. In particular, we have*

$$\partial y_i^*/\partial r_i \gtreqless 0 \quad \textit{as} \quad r_i \lesseqgtr r_j \frac{(A+1)R+1}{A} \quad \textit{where } i \neq j.$$

THREE-CANDIDATE COMPETITION: TWO MAIN CANDIDATES AND A SPOILER

Now consider candidate competition with three candidates, labeled 1, 2, and 3. We shall examine a special case in which candidate 3 has sufficiently low initial support that candidates 1 and 2 do not consider 3 a threat. These two main candidates have similar payoff functions to those in the two-candidate case ($U^1 = r_1^0 - r_2^0$ and $U^2 = r_2^0 - r_1^0$), whereas candidate 3, the spoiler, maximizes just his or her own percentage support ($U^3 = r_3^0$).

Candidates 1 and 2 allocate their effort between positive campaigning just as they do in the two-candidate cases and described in equation 1; there is no issue of attacking candidate 3, since they do not consider 3 a threat.[10] Moreover, because negative campaigning harms the attacker and candidate 3 cares only about his or her own support, candidate 3 would not want to engage in negative campaigning and would put all effort into positive campaigning (i.e., $y_3 = 1$ regardless of the choices of the two other candidates). Thus the first three-candidate model is the two-candidate model with the difference that some currently undecided voters will go to a third candidate. Allowing for this difference requires the specification of a sharing function among three, instead of just two, candidates. For any given triple (y_1, y_2, y_3) of positive campaigning efforts, let $q^i(y_1, y_2, y_3)$ denote the share of undecided voters received by candidate *i* (= 1, 2, 3). We maintain the following properties:

Assumption 7. $q^i(y_1, y_2, y_3)$ *is increasing in* y_i *and decreasing in* y_j *for* i ≠ j *for all* i.

Assumption 8. *Interchanging the positive campaigning efforts of two candidates is equivalent to an interchange of their shares of undecided voters.*

Assumption 9. $q^1(y_1, y_2, y_3) + q^2(y_1, y_2, y_3) + q^3(y_1, y_2, y_3) = 1$.

Note that these properties mirror assumptions 1–3 of the two-candidate case, with assumptions 8 and 9 implying $q^i(y, y, y) = q^j(y, y, y) = 1/3$ for all *i*, *j* and for all *y*. (In words: If all three candidates put the same amount of effort, the shares of undecided voters received are the same for all the candidates).

With $y_3 = 1$, the payoff functions of the three candidates are

$$\begin{aligned} U^1(y_1, y_2, 1) &= [q^1(y_1, y_2, 1) - q^2(y_1, y_2, 1)]R - B[1 - y_1 \\ &\quad + A(1 - y_2)]r_1 + B[1 - y_2 + A(1 - y_1)]r_2 + r_1 - r_2 \\ U^2(y_1, y_2, 1) &= [q^2(y_1, y_2, 1) - q^1(y_1, y_2, 1)]R - B[1 - y_2 \\ &\quad + A(1 - y_1)]r_2 + B[1 - y_1 + A(1 - y_2)]r_1 + r_2 - r_1 \\ U^3(y_1, y_2, 1) &= q^3(y_1, y_2, 1)R + r_3. \end{aligned} \qquad \textbf{(16)}$$

An equilibrium for the strategic form game with these payoff functions is defined similarly to equation

5. Such an equilibrium is shown to exist, under a condition similar to assumption 5, in theorem 2. For comparison purposes here, we examine the equilibrium under the following adaptation of assumption 6:

Assumption 10. $q^i(y_1, y_2, y_3) =$

$$\frac{y_i}{y_1 + y_2 + y_3} \quad \textit{for all } i = 1, 2, 3.$$

In an interior equilibrium $(\bar{y}_1, \bar{y}_2)$, we have

$$U_1^1(\bar{y}_1, \bar{y}_2, 1) = 0 \quad \text{and} \quad U_2^2(\bar{y}_1, \bar{y}_2, 1) = 0,$$

which, under assumption 10, become

$$\frac{2\bar{y}_2 + 1}{\bar{y}_1 + \bar{y}_2 + 1} R = B(Ar_2 - r_1) \qquad \textbf{(17)}$$

$$\frac{2\bar{y}_1 + 1}{\bar{y}_1 + \bar{y}_2 + 1} R = B(Ar_1 - r_2), \qquad \textbf{(18)}$$

respectively. In turn, dividing equation 17 by equation 18 yields

$$\frac{2\bar{y}_2 + 1}{2\bar{y}_1 + 1} = \frac{Ar_2 - r_1}{Ar_1 - r_2}.$$

Without loss of generality suppose $r_1 > r_2$ (candidate 1 is the front-runner). Then, since $Ar_2 - r_1 < Ar_1 - r_2$, the above equation implies $\bar{y}_1 > \bar{y}_2$. Thus, as in proposition 3, *the front runner continues to engage in more positive campaigning than the main opponent.*

Solving equations 17–18 yields

$$\bar{y}_1 = \frac{2(1 - r_1 - r_2 - r_3)(Ar_1 - r_2)}{B(r_1 + r_2)^2 - (A - 1)^2} - 1/2 \qquad \textbf{(19)}$$

$$\bar{y}_2 = \frac{2(1 - r_1 - r_2 - r_3)(Ar_2 - r_1)}{B(r_1 + r_2)^2(A - 1)^2} - 1/2. \qquad \textbf{(20)}$$

Note that the first terms of equations 19 and 20 with $r_3 = 0$ are equal to y_1^* and y_2^*, respectively—the equilibrium positive campaigning efforts reported in equations 14–15 for the case of pure two-candidate competition. Consequently, the presence of the third spoiler candidate reduces positive campaigning of both main candidates. The reason for this lower positive campaigning for a given share of undecided voters is that its marginal return is reduced for the two main candidates, since candidate 3 puts all his effort into positive campaigning and takes a part of the undecided votes, thus reducing the total number of undecided voters left to be captured by candidates 1 and 2. The two main candidates then find negative campaigning relatively more profitable. Note also from equations 19–20 that for the two main candidates to put at least some effort into positive campaigning ($\bar{y}_1, \bar{y}_2 > 0$), y_1^* and y_2^* must be greater than 1/2. That is, the conditions for an interior equilibrium are more stringent in this case. Finally, since $\bar{y}_1$ and $\bar{y}_2$ differ from y_1^* and y_2^*, respectively, by only a constant when R $(= 1 - r_1 - r_2 - r_3)$ is fixed, the comparative static results reported in proposition 4 are also valid here. We summarize these results as proposition 5:

Proposition 5. *Suppose a long-shot third candidate enters the competition between the two main candidates, with the payoff functions described in equation 16, and make assumption 6. Then:*

a. *The long-shot candidate only engages in positive campaigning, whereas the two main candidates intensify their negative campaigning.*
b. *All the results reported in proposition 4 continue to hold for the two main candidates.*

We should emphasize that although the entrance of the long-shot candidate increases the two main candidates' negative campaigning, the overall ratio of negative to positive campaigning could well decrease since the long-shot candidate engages solely in positive campaigning. In addition, the results reported in proposition 5 do not have to be valid in general three-candidate contests to which we now turn.

THREE-CANDIDATE COMPETITION: A GENERAL MODEL

In some elections, especially primaries, there are more than two candidates who at the outset have a realistic chance of winning. In the course of such campaigns, the phenomenon of "ganging on the front-runner"—all the other candidates attacking the front-runner— is often observed. We shall examine a general three-candidate model that includes the model of the previous section as a special case and that helps us understand those concerted attacks against front-runners. The model and the results can easily be extended to contests with any number of candidates, but at additional notational burden.

Again, we label the candidates, 1, 2, and 3. Each candidate i allocates effort among positive campaigning (y_i), negative campaigning against candidate j (x_i^j), and negative campaigning against candidate k (x_i^k), where $i \neq j \neq k$. Give that each candidate has one unit of resource, the constraint faced by each one of them is

$$1 = x_i^j + x_i^k + y_i \quad \text{for all} \quad i = 1, 2, 3 \quad \text{and} \quad i \neq j, k. \qquad \textbf{(21)}$$

As before, positive campaigning determines the share of undecided voters won over by each candidate, with $q^i(y_1, y_2, y_3)$ denoting the share of candidate i for any positive campaigning triple (y_1, y_2, y_3). We assume the q^is satisfy assumptions 7–9. A vector of negative campaigning efforts $(x_1^2, x_1^3, x_2^1, x_2^3, x_3^1, x_3^2) \equiv x$, reduces the support of candidates by turning some supporters into undecided voters with the percentage reduction in support for candidate i equaling $B[x_i^j + x_i^k + A(x_j^i + x_k^i)]r_i$. Thus the new support level of candidate i becomes

$$r_i^0 = r_i + q^i(y_1, y_2, y_3)R - B[x_i^j + x_i^k + A(x_j^i + x_k^i)]r_i. \qquad \textbf{(22)}$$

(For brevity, we omit error terms.) The payoffs of the three candidates as functions of the new support levels have the following form:

$$\begin{aligned} W^1 &= r_1^0 - \alpha_2 r_2^0 - \alpha_3 r_3^0 \\ W^2 &= r_2^0 - \beta_1 r_1^0 - \beta_3 r_3^0 \\ W^3 &= r_3^0 - \gamma_1 r_1^0 - \gamma_2 r_2^0. \end{aligned} \tag{23}$$

The parameters can take values between 0 and 1, and they represent weights attached by each candidate to the expected support levels of their opponents. That is, the parameters reflect the extent to which a candidate might be more concerned with the vote share and the likely success of one candidate as opposed to another. The model of the previous section is obtained from equation 23 by setting $\alpha_2 = \beta_1 = 1$ and $\alpha_3 = \beta_3 = \gamma_1 = \gamma_2 = 0$ (i.e., the two main candidates care symmetrically about each other's vote shares, whereas the third candidate cares only about how well he or she personally does.) In some cases, the weights could be interpreted as probabilities. For example, with $\alpha_2 = 1 - \alpha_3 = \alpha$, $\beta_1 = 1 - \beta_3 = \beta$, and $\gamma_1 = 1 - \gamma_2 = \gamma$, we can interpret equation 23 as follows: all candidates believe $\text{Prob}(r_2^0 > r_3^0) = \alpha$, $\text{Prob}(r_1^0 > r_3^0) = \beta$, and $\text{Prob}(r_1^0 > r_2^0) = \gamma$, with candidate 1 maximizing $W^1 = \alpha(r_1^0 - r_2^0) + (1 - \alpha)(r_1^0 - r_3^0)$ and similarly for the two other candidates.[11] By substituting equation 22 into equation 23 and taking into account equation 21, the payoffs can be expressed as functions of $x = (x_1^1, x_1^3, x_2^1, x_2^3 x_3^1, x_3^2)$ [we let $y \equiv (1 - x_1^2 - x_1^3, 1 - x_2^1 - x_2^3, 1 - x_3^1 - x_3^2)$]:

$$\begin{aligned} W^1(x) &= r_1 - \alpha_2 r_2 - \alpha_3 r_3 + [q^1(y) - \alpha_2 q^2(y) - \alpha_3 q^3(y)]R \\ &\quad + B\{[x_2^1 + x_x^3 + A(x_1^2 + x_3^2)]a_2 r_2 + [x_3^1 + x_3^2 + A(x_1^3 \\ &\quad + x_2^3)]\alpha_3 r_3 - [x_1^2 + x_1^3 + A(x_2^1 + x_3^1)]r_1\} \\ W^2(x) &= r_2 - \beta_1 r_1 - \beta_3 r_3 + [q^2(y) - \beta_1 q^1(y) - \beta_3 q^3(y)]R \\ &\quad + B\{[x_1^2 + x_1^3 + A(x_2^1 + x_3^1)]\beta_1 r_1 + [x_3^1 + x_3^2 + A(x_1^3 \\ &\quad + x_2^3)]\beta_3 r_3 - [x_2^1 + x_2^3 + A(x_1^2 + x_3^2)]r_2\} \\ W^3(x) &= r_3 - \gamma_1 r_1 - \gamma_2 r_2 + [q^3(y) - \gamma_1 q^1(y) - \gamma_2 q^2(y)]R \\ &\quad + B\{[x_1^2 + x_1^3 + A(x_2^1 + x_3^1)]\gamma_1 r_1 + [x_2^1 + x_2^3 + A(x_1^2 \\ &\quad + x_3^2)]\gamma_2 r_2 - [x_3^1 + x_3^2 + A(x_1^3 + x_2^3)]r_3\}. \end{aligned} \tag{24}$$

To guarantee a pure-strategy equilibrium, we introduce the following three-candidate analogue to assumption 5 (i.e., a candidate's marginal return to positive campaigning is diminishing as the candidate engages in more negative campaigning):

Assumption 11. $q^i(y_1, y_2, y_3)$ *is a twice-differentiable concave function of* y_i *for all* i = *1, 2, 3.*

Theorem 2. *For three-candidate competition, make assumptions 7–9 and 11. Then the payoff function of each candidate is concave in the candidate's own strategy vector and a pure-strategy equilibrium exists.*

For the remainder of this section and without loss of generality, suppose $r_1 > r_2 > r_3$ and consider the following property:

Assumption 12. $\gamma_1 r_1 > \gamma_2 r_2$, $\beta_1 r_1 > \beta_3 r_3$, *and* $\alpha_2 r_2 > \alpha_3 r_3$.

Consider the first inequality in assumption 12. The two parameters, γ_1 and γ_2, are weights attached by candidate 3 to the initial support levels of candidates 1 and 2, respectively. Given the convention of $r_1 > r_2$, the inequality states that the weight attached by candidate 3 to the stronger opponent (γ_1) should be sufficiently high relative to the weight attached to the weaker opponent. In particular, rewriting the inequality as $\gamma_1/\gamma_2 > r_2/r_1$, it states that the ratio of weights should be greater than the inverse ratio of initial supports. A candidate who weighs the stronger supporter more than the weaker one always satisfies this property. The two other inequalities in assumption 12 have a similar interpretation.

Proposition 6. *Make assumptions 7–9 and 11–12 and consider any equilibrium strategy combination* x*. *Then, no candidate engages in negative campaigning against the weaker of two opponents (i.e., under the convention* $r_1 > r_2 > r_3$, *we have* $x_1^{3*} = x_2^{3*} = x_3^{2*} = 0$*).*

An immediate implication of this proposition is that if the two weaker candidates were to engage in negative campaigning they would both attack the front-runner, the phenomenon frequently observed in primary multicandidate elections. If the front-runner were to engage in negative campaigning, it would be directed against the stronger opponent.

To see the logic of this proposition, note that for a candidate to engage in negative campaigning against one opponent, the marginal cost of the last extra unit of negative campaigning must be equated to its marginal benefit. In equilibrium, the marginal cost of negative campaigning against either of a candidate's opponents is the same: it is the marginal return to positive campaigning. On the other hand, the marginal benefits of negative campaigning are different because they take into account the initial support level of each opponent and the weight attached to the support of that opponent by the candidate in question. Given the constancy of the damage inflicted by negative campaigning (the parameters *B* and *A*), it then always pays to put all negative campaigning effort, if any, against the opponent with higher marginal benefits, that is, the stronger opponent. Thus if every candidate were to engage in at least some negative campaigning, candidates 2 and 3 would concentrate their attacks against candidate 1 and candidate 1 would attack candidate 2; nobody would attack the weakest candidate 3.

These stark results reported in proposition 6 would not always survive changes in the specification of our model. Most obviously, if a candidate were more vulnerable to negative campaigning on some aspect of his history or issue positions than others, then we could have negative attacks against a weaker candidate with a comparatively large vulnerability.

We could model the different vulnerabilities by, for example, introducing a different B for attacks against each different candidate. Consider, for example, the limiting case with $B = 0$ in equation 22 for candidate 1 (i.e., he is completely invulnerable to attacks), but with $B > 0$ for candidates 2 and 3. Then, by appropriately modifying the payoff functions in equation 24 and performing a similar exercise to that in the proof of proposition 6, it can be easily shown that candidate 1 is never attacked by the other two (weaker candidates). Depending on the values of the parameters in their payoff functions, candidates 2 and 3 might or might not engage in negative campaigning against each other. In other words, as with other results in the paper, proposition 6 should not be interpreted as saying that attacks against weaker candidates should never occur in practice but, rather, that there is a tendency for not attacking weaker opponents in three-way contests.

DISCUSSION

We see our work as a contribution to two different research traditions. On the one hand, there is a considerable body of formal deductive modeling, largely inspired by rational choice ideas, that models campaign competition as a (sequential) process of decision making by candidates about questions such as what policy positions to offer and on how best to spend campaign funds.[12] The present effort is directly complementary to recent work in that tradition, most importantly to that of Harrington and Hess (1993).[13] On the other hand, we also see our endeavor as a contribution to the (largely empirical) literature on public opinion and political persuasion (e.g., Bartels 1993; Franklin 1991; Patterson and McClure 1973; Popkin 1991; Zaller 1992), including important recent experimental work on campaign advertising and rhetoric (Ansolabehere and Iyengar 1991, 1993; Ansolabehere et al. 1994; Iyengar and Kinder 1978; Roddy and Garramone 1988), to which we see ourselves as contributing theoretically significant and testable hypotheses. The original inspiration for the models developed herein derives from two sources, one theoretical and one empirical.

The theoretical source was a seemingly paradoxical result in game theory concerning three-way duels, or *truels*.[14] Often the most accurate duelist has a lower probability of survival than the second-best (or even third-best) shooter. The reason for this result is that the optimal strategy of either of the other duelists is to shoot at the duelist who is the best shot. We began with a trueling model but quickly realized that the assumptions that were peculiar to campaign competition led to results about three-candidate competition in single-member plurality elections that were only tangentially related to earlier results about three-way duels. Nevertheless, our general three-candidate model yields insights similar to that in truels: to the extent there is negative campaigning, the two weaker candidates attack the front-runner and not each other.

The empirical trigger for our work was the discussion by journalists and media pundits of the use and nonuse of negative campaigning in the 1992 presidential campaign, in particular their attempts to explain why both Bush and Clinton appeared reluctant to attack Perot. Journalists who have analyzed the 1992 election offered as a key explanation of the unwillingness of Bush and Clinton to attack Perot personally that neither front-runner wished to be the first to attack Perot because they wished to let sleeping dogs lie and feared that Perot was the kind of person who would retaliate with direct attacks on the candidate who first attacked him even if that cost Perot support. While we do not dispute that this explanation may have been relevant to the calculations of the Democratic and Republican presidential nominees, we are suspicious of explanations that rely *largely* on idiosyncratic features of one of the candidates to account for the key features of the overall dynamic of the campaign when there are regularities that may apply across campaigns when we identify relevant variables and model their impact.

We believe that the model we have proposed (although it is clearly simplified) can nonetheless help us to account for important parts of the 1992 campaign dynamic without requiring us to incorporate features of the election tied to Perot's unique campaign style and personality. Our model calls attention to the very different strategies in a three-way race that makes sense for front-runners and near front-runners, as opposed to strategies that make sense for the candidate who is a distant third.[15] In particular, our three-candidate model with a "spoiler" candidate can help account for (1) why Bush and Clinton as front-runners directed their attacks primarily against each other and not against Perot,[16] (2) why Perot's reentry into the race raised the negativity of the front-runners' campaigning (at least initially),[17] and (3) why it made sense for Perot to concentrate so much on positive campaigning.

But our model has specific empirical implications that are broader than any single campaign. The results are compatible with commonsense intuitions and consistent with a considerable body of evidence that incumbents (who are often front-runners) are much less likely to engage in negative campaigning than are challengers—such as the finding by Kaid and Davidson (1986), in a study of 1982 contests for the U.S. Senate, that only 10% of incumbent ads were negative but 46% of the challenger were negative.[18] Similarly, our results help us account for a pattern, frequently been observed by journalists, of increases in negativity of campaign tactics in two-candidate contests by a candidate whose support is eroding—as occurred in the 1988 presidential campaign when Dukakis' lead shrank as a result of Bush attack ads and in the 1992 Senatorial election in California, when Barbara Boxer became more negative in her ads as Bruce Herschensohn narrowed her initially very large lead. In addition, our results in the general three-candidate model (which can easily be extended to a greater number of candidates) are consistent with the "ganging up on the front-runner" pattern found in numerous primaries and nonpartisan elections such as the anti-Dukakis coalition of 1988 and the anti-(front-runner) Michael Woo stance of many candidates in the 1992 Los Angeles mayoral competition that allowed Richard Riordan to emerge into the runoff relatively unscathed.

Of course, in no way are we claiming that our modeling of the factors influencing negative campaigning is the last word, and we have treated the actual psychological processes of political persuasion as a "black box." Moreover, while the model seems to capture many of the critical elements of the incentive structure for negative campaigning, there are limitations on its ability to capture important subleties. In particular, if we treat *any* evidence to the contrary as disconfirmation even if the overall pattern is one that generally supports the results of the model, there are some predictions of our model that are falsified in the 1992 election. For example, according to a strict interpretation of our model, there should have been no negative campaigning directed against Perot by either Bush or Clinton in the last phase of the campaign when Perot was clearly running third. Yet at the very end of the campaign Bush did attack Perot, referring to him (along with Clinton) as a "bozo." This might have been precipitated by Perot's reporting of stories of plots and threats (such as the one allegedly centered around his daughter's wedding) which damaged his own credibility, almost certainly made him more vulnerable to negative campaigning, and also reduced the probable voter backlash against a front-runner who might choose to attack him. As we indicated, such peculiarities of a campaign could be captured by assigning different vulnerability parameters to different candidates. Although we believe individual candidate characteristics are important in actual campaigns, we have here largely abstracted from them in order to concentrate on tendencies and regularities in negative campaigning that hold independently of individual candidate attributes.

A number of ideas for possible modification to our assumptions have occurred to us. One is to allow for certain "last period" effects, where a candidate who is running second will take desperation measures (since, with loss of the election expected, he has little to lose by strategic experimentation) or will engage in more negative attacks because there is no time for rebuttal. As the model now stands, the last period of a campaign is treated the same way as any other period. Modifying this assumption, however, by allowing different ways of determining the votes of the undecided voters in the last period does not appear to change the qualitative results of two-candidate competition or that of thee candidates with a "spoiler" candidate. Nor, we conjecture, would our results change in the general three-candidate contest even if we were to include last-period effects. We plan to examine this in a subsequent paper.

Another assumption that could be modified has to do with the function that assigns expected gains and losses of various campaign strategies. The model posits that the expected consequences of an attack do not depend upon whether or not it is seen as an unprovoked first strike, do not allow the consequences of an attack to vary with the number of attacks that have previously been made, and do not include effects on voter turnout. Moreover (as noted), we have not incorporated candidate-specific vulnerabilities, although we can do so straightforwardly with predictable results.[19]

However, just as Downs's work on spatial models of political competition launched a body of research that has enhanced our understanding of the electoral process, so we hope that the present paper will lead to more interest by political scientists in describing and modeling negative campaigning and other aspects of political debate. We believe that political persuasion is an important issue that has been little studied by those working in the rational choice tradition. We should note that Downs himself, in perhaps the least cited aspect of his classic work emphasized the importance of political persuasion (1957, 83, 84; see also Grofman and Withers 1993; Weatherford 1993). Nonetheless, the simple median voter story of candidate competition that passes for Downs's view in some American government textbooks has the candidates offering issue positions among which voters choose, with voter choice based simply on voter's relative proximity to platforms proposed by the candidates. Missing from that story are at least four critical elements of politics: (1) the information conveyed by candidates is not only about putative issue positions but also about candidate attributes such as competence and trustworthiness; (2) almost invariably, candidates not only describe themselves and their own policy positions but also seek to (mis)characterize their opponent and their opponent's policy positions as well; (3) in a world of multidimensional issue competition, candidates not only seek to convey the positions they wish to be attributed to themselves and to their opponent but also often seek to persuade voters that some dimensions (some issues) are more important than others; and (4) voters do not believe all of what they are told. Here, we have sought to develop an approach that recognizes that fact that *competing* stories are being told, not just a story each candidate tells about his own policy positions or his own office-worthiness. We see the present essay as a beginning of such modeling efforts for the understanding of one important aspect of political persuasion—the consequences of negative campaigning and the incentives for its use.

APPENDIX A: SYMBOLS

x_i	positive campaigning by candidate i
y_i	negative campaigning by candidate i
r_i	initial support for candidate i (in fractions)

$R = 1 - r_1 - r_2$	share of undecided voters
$q^i(y_1, y_2)$	positive campaigning sharing function for candidate i
$q(\cdot, \cdot)$	positive campaigning sharing function satisfying assumptions 1–3
B	overall effectiveness of negative campaigning
A	relative effectiveness of negative campaigning
r_1^0	new support for candidate i (in fractions)
V^i	payoff function of candidate i in two-candidate competition
V_j^i	$\equiv \partial V^i(y_1, y_2)/\partial y_j$
q_i	$\equiv \partial q(y_1, y_2)/\partial y_i$
$\rho i(y)$	reaction function of candidate i
U^i	payoff function of candidate i in competition with two main candidates and a spoiler
W^i	payoff function of candidate i in general three-candidate competition
x_i^J	negative campaigning of candidate i against candidate j (three-candidate competition)

APPENDIX B: PROOFS

Proof of Theorem 1. To show existence of a pure-strategy equilibrium, it is sufficient to show that the payoff function of each candidate is concave in the candidate's own strategy, that is, we just need to show that V^1 is concave in y_1 and V^2 is concave in y_2. Let $V_1^1 \equiv \partial V^1(y_1, y_2)/\partial y_1$ and let V_2^2 be similarly defined. From the payoff functions in equation 4, it is straightforward to derive the following expressions:

$$V_1^1 = 2q_1R - B(Ar_2 - r_1)$$

$$V_2^2 = -2q_2R - B(Ar_1 - r_2),$$

(where $q_i = \partial q/\partial y_i$). Defining V_{11}^1 and V_{22}^2 similarly, we have $V_{11}^1 = 2q_{11}R$ and $V_{22}^2 = -2q_{22}R$. By assumption 5, V_{11}^1 and V_{22}^2 are both nonpositive, and so V^1 and V^2 are concave as required. Q.E.D.

LEMMA 1. *Make assumptions 1–3 and 5 and* $q_{12}(w, z) \neq 0$ *when* $w \neq z$. *Then* $q_1(y_1, y_2) < -q_2(y_1, y_2) \leftrightarrow q(y_1, y_2) > 1/2$.

Proof of Lemma 1. ($\leftarrow$) Suppose $q(y_1, y_2) > 1/2$ ($\leftrightarrow y_1 > y_2$). (Note that we have used assumptions 1–3 in expressing $q^1(y_1, y_2)$ as $q(y_1, y_2)$ and $q^2(y_1, y_2)$ as $q(y_2, y_1)$.) Then by the concavity of the sharing function in the candidate's own positive campaigning, assumption 5 implies

$$q_1(y_1, y_2) \leq q_1(y_2, y_2) \quad \text{and} \tag{25}$$

$$-q_2(y_1, y_2) \geq -q_2(y_1, y_1). \tag{26}$$

Given $q_{12}(w, z) \neq 0$ when $w \neq z$ (and since $y_1 \neq y_2$), we examine the following two cases:

Case 1: $q_{12}(y_1, y_2) > 0$.
Then, given $y_1 > y_2$, we have $q_2(y_1, y_2) < q_2(y_2, y_2)$ which implies $-q_2(y_1, y_2) > -q_2(y_2, y_2)$. Since $-q_2(y_2, y_2) = q_1(y_2, y_2)$, by equation 25 we then have $-q_2(y_1, y_2) > q_1(y_1, y_2)$.

Case 2: $q_{12}(y_1, y_2) < 0$.
This case implies that $q_1(y_1, y_2) < q_1(y_1, y_1) = -q_2(y_1, y_1)$ which by equation 26 is less than or equal to $-q_2(y_1, y_2)$, or that $q_1(y_1, y_2) < -q_2(y_1, y_2)$ as stated in the lemma.

($\rightarrow$) This can be shown, using the same steps as above, that $q(y_1, y_2) \leq 1/2$ implies $q_1(y_1, y_2) \geq -q_2(y_1, y_2)$. Q.E.D.

Proof of Proposition 4. Parts a, b, and d are straightforward. To show part c, use equations 14–15 to derive

$$\begin{aligned}\partial y_1^*/\partial A &= \frac{2Rr_1}{B(r_1 + r_2)^2(A-1)^2} - \frac{4R(Ar_1 - r_2)}{B(r_1 + r_2)^2(A-1)^3}\\ &= \frac{2Rr_1(A-1)}{B(r_1 + r_2)^2(A-1)^3} - \frac{4R(Ar_1 - r_2)}{B(r_1 + r_2)^2(A-1)^3}\\ &= \frac{2R[r_1(A-1) - 2(Ar_1 - r_2)]}{B(r_1 + r_2)^2(A-1)^3}\\ &= \frac{2R[2r_2 - (A+1)r_1]}{B(r_1 + r_2)^2(A-1)^3}.\end{aligned}$$

From the last expression it follows that $\partial y_1^*/\partial A \lesseqgtr 0$ as $2r_2 - (A + 1)r_1 \lesseqgtr$ or, as $r_1 / r_2 \gtreqless 2/(A + 1)$. Showing the expression for y_2^* follows similar steps, a process we will not repeat here.

As for part e, from equation 14 we have

$$\begin{aligned}\partial y_1^*/\partial r_1 &= \frac{-2(Ar_1 - r_2) + 2A(1 - r_1 - r_2)}{B(r_1 + r_2)^2(A-1)^2}\\ &\qquad - \frac{4(1 - r_1 - r_2)(Ar_1 - r_2)}{B(r_1 + r_2)^3(A-1)^2}\\ &= \frac{2}{B(r_1 + r_2)^3(A-1)^2}[-(Ar_1 - r_2)(r_1 + r_2) +\\ &\qquad A(1 - r_1 - r_2)(r_1 + r_2) - 2(Ar_1 - r_2)(1 - r_1 - r_2)].\end{aligned}$$

It follows that $\partial y_1^*/\partial r_1 \gtreqless 0$ as the last expression inside the brackets is positive or negative. After some tedious algebra, this expression simplifies into

$$\begin{aligned}(A + 1)r_2(1 - r_1 - r_2) + r_2 - Ar_1 &=\\ &(A + 1)r_2R + r_2 - Ar_1.\end{aligned}$$

Thus $\partial y_1^*/\partial r_1 \gtreqless 0$ as $(A + 1)r_2R + r_2 - Ar_1 \gtreqless 0$ or as

$$r_1 \lesseqgtr r_2 \frac{(A+1)R + 1}{A}.$$

The equivalent expression for $\partial y_2^*/\partial r_2$ is similarly derived. Q.E.D.

Proof of Theorem 2. Candidate's i strategy vector is (x_i^j, x_i^k). Concavity of candidate i's payoff function in that vector implies existence of pure-strategy equilibrium; thus this is what we need to show. For specificity, consider candidate 1's payoff function and let $W^1_{23} \equiv \partial^2 W^1/\partial x_1^2 \partial x_1^3$, $W^1_{22} \equiv \partial^2 W^1/(\partial x_1^2)^2$, $W^1_{33} \equiv \partial^2 W^1/(\partial x_1^3)^2$. It is straightforward to show that

$$W^1_{23} = W^1_{22} = W^1_{33} = (q^1_{11} - \alpha_2 q^2_{11} - \alpha_3 q^3_{11})R,$$

where $q^i_{11} = \partial^2 q^1(y_1, y_2, y_3)/\partial y_1^2$ for 1 = 1, 2, 3. By assumption 11, the value of any of these second derivatives is nonpositive. Then, the following quadratic form

$$[z_1 \quad z_2]\begin{bmatrix} W^1_{22} & W^1_{23} \\ W^1_{23} & W^1_{33} \end{bmatrix}\begin{bmatrix} z_1 \\ z_2 \end{bmatrix}$$

equals $(q^1_{11} - \alpha_2 q^2_{11} - \alpha_3 q^3_{11})R(z_1 + z_2)^2$, which is nonpositive for all vectors (z_1, z_2). Thus, the Hessian (the matrix of second derivatives) of W^1 with respect to candidate 1's strategy vector is negative semidefinite, which implies that concavity of W^1 is candidate in his or her strategy. Showing the same result for candidates 2 and 3 follows exactly the same steps.

Q.E.D.

Proof of Proposition 6. We consider only the case of candidate 1; the proof of the two other cases follows the same steps. First, suppose $x_1^{2*} \in [0, 1)$. Then, the following condition must hold:

$$\frac{\partial W^1(x^*)}{\partial x_1^2} = -[q^1_1(y^*) - \alpha_2 q^2_1(y^*) - \alpha_3 q^3_1(y^*)]R + B[A\alpha_2 r_2 - r_1] \le 0, \quad (27)$$

where $y^* \equiv (1 - x_1^{2*} - x_1^{3*}, 1 - x_2^{1*} - x_2^{3*}, 1 - x_3^{1*} - x_3^{2*})$. The derivative of candidate 1's payoff function with respect to negative campaigning against the weaker candidate 3 is

$$\frac{\partial W^1(x^*)}{\partial x^3} = -[q^1_1(y^*) - \alpha_2 q^2_1(y^*) - \alpha_3 q^3_1(y^*)]R + B[A\alpha_3 r_3 - r_1] \le 0. \quad (28)$$

The first terms of equations 27 and 28 are identical; and since, by assumption 12, $\alpha_2 r_2 > \alpha_3 r_3$, the second term of equation 28 is smaller than the second term of equation 27, or $B(A\alpha_3 r_3 - r_1) < B(A\alpha_2 r_2 - r_1)$. Thus we have

$$\frac{\partial W^1(x^*)}{\partial x_1^3} < \frac{\partial W^1(x^*)}{\partial x_1^2} \le 0,$$

which, given the concavity of $W^1(x)$ in x_1^3, implies $x_1^{3*} = 0$.

Finally, suppose $x_1^{2*} = 1$. Obviously, given the constraint in equation 21, we cannot have anything else but $x_1^{3*} = 0$. Therefore, regardless of the value of x_1^{2*}, we have $x_1^{3*} = 0$ as stated in the proposition.

Q.E.D.

Notes

We would like to thank Joe Harrington, and participants in the Public Choice study group at the University of California, Irvine for helpful comments. Skaperdas thanks the National Science Foundation for financial support.

1. Similar or identical definitions of negative campaigning have been used by campaign consultants (Tarrance 1982) and in survey-based studies of negative campaigning (Garramone 1984). We would, however, follow Johnson-Cartee and Copeland in also including within the rubric of negative ads those in which the negativity is left to inference, such as Johnson's famous "Daisy" ad in 1964 (1991, 17). Other definitions of negative campaigning that have been proposed are narrower. For example, Merritt asserts that only ads that focus "primarily on degrading perceptions of the rival" should be classified as negative (1984, 27). We have deliberately chosen a more general definition of negative campaigning so as to include both policy aspects of political communication and aspects that deal with candidate attributes (e.g., attacks directed at an opposing candidate's personality, morality of record) that are only incidentally relevant to identifying areas of policy difference between a candidate and an opponent. Even the physical appearance of a candidate can become a feature that affects voters' judgments and that enters into negative campaigning (Rosenberg et al. 1986). Although some aspects of a candidate's history or personality will be seen by (some) voters as relevant to whom they should choose even though such candidate features cannot be directly tied to particular policy positions (e.g., claims about, say, what Ted Kennedy did or did not do at Chappaquidick), in general it is difficult to distinguish between attacks against character that have policy implications and those that do not. Thus we use a simple dichotomy between statements in praise of one's past history and present policy positions (positive campaigning) versus attacks directed in some fashion *against* one's opponent or his policy positions (negative campaigning). Alternatively, following Harrington and Hess (1993) we might model political competition as having two dimensions: a policy dimension and a "valence" dimension where the personal attributes of candidates are evaluated.

2. See discussion in Pfau and Kenski (1990, 2–3 and chap. 2). Psychologists have generally found that "not only is negative information more heavily weighted than positive information in the initial formation of impressions, but negative information exhibits a greater capacity to alter already existing impressions" (Kellerman 1984, 37–38). Lau (1985) has examined the differential impact of negative and positive information as it pertains to politics. Pfau and Kenski take negative campaigning very seriously; indeed, the central focus of the latter part of their book is on how to mitigate the more pernicious effects of negative campaigning through the use of "inoculation" strategies.

3. However, the existence of the boomerang effect is not unchallenged. Johnson-Cartee and Copeland note that political consultants tend to dismiss the boomerang effect and claim that even if there is a fall off in support for the candidate engaging in attack ads, that fall off is very short-run (1991, 14–15). All the qualitative results we present below do not depend on incorporating the boomerang effect in our model. In its absence we would just obtain higher levels of negative campaigning.

4. While Garramone (1985) finds that political advertising had a strong negative impact on respondent's evaluation of the sponsor of the negative ads they remember and only a slight negative influence on the evaluation of the target of the ads, this is just one study and we believe that the assumption that we have made is more compatible with the prevalence of negative advertising. If it were true that negative campaigning generally hurt the sponsor more than the victim, it is hard to see how negative ads with attributed candidate sponsorship would ever be used. Of course, some negative ads may hurt the sponsor more than the target. And certain types of attack may be delegated or otherwise left to organizations not

directly linked to the candidate or be left to lesser spokespersons, and thus remain disavowable.

5. Candidate decision making can be seen as part of a complex ongoing game in which candidates are seeking not only to influence voters directly but also to affect the decisions being taken by activists and interest groups about their levels of campaign participation and contributions, and the decisions of journalists as to the nature and extent of campaign coverage—decisions that can have a tremendous (indirect) impact on the candidate's election chances. Thus certain campaign decisions may be taken for reasons that are linked in quite complex ways to a candidate's ultimate goal of influencing decisions by the voters as to whether or not to vote and about which candidate to support if they do vote. Here, to make the analysis tractable, we abstract away from such institutional richness of detail.

6. A more general alternative is $q^1(y_1, y_2) + q^2(y_1, y_2) + \bar{q}(y_1, y_2) = 1$, where $\bar{q}(y_1, y_2)$ denotes the fraction of undecided voters remaining undecided. A reasonable property of $\bar{q}(\cdot)$ is that it is decreasing (and symmetric) in its arguments (*i.e., the more positive campaigning there is, the smaller the* proportion of voters remaining undecided). Our results would be unaffected with this modification and the companion property to assumption 5 introduced later on.

7. For a derivation of this result in another context, see Skaperdas 1994, theor. 1. Functions satisfying assumptions 1–3 have been used in various areas of several social science disciplines, including voting (Coughlin 1986), rent seeking (Tullock 1980), and conflict (Skaperdas 1992). For a general axiomatic development see Luce and Suppes 1965 or Suppes et al. 1989. For a comparison of the properties of two functional forms, see Hirshleifer (1989). In all of these cases, the functions have a probabilistic interpretation. We can also interpret here these functions as probabilistic at the individual voter level but maintain a deterministic interpretation at the aggregate level by assuming a large number of voters.

8. Given equation 3, $V^i = E(r_i^0) - E(r_j^0)$ is equivalent to maximizing $\text{Prob}(r_i^0 > r_j^0)$ when the error terms are independent of the strategies of the candidates (see Aranson, Hinich, and Ordeshook 1974, theor. 4). See also Hinich and Ordeshook 1970 for an earlier treatment of the problem of payoff-function equivalence.

9. The reaction function (or the best-reply or best-response function) of a candidate describes the optimal strategy of that candidate for any given strategy of the opponent. Candidate 1's reaction function is derived from equation 11 by expressing y_1 as a function of y_2 and the other parameters. Candidate 2's reaction function is similarly derived from equation 12.

10. In fact, we shall show the optimality of never engaging in negative campaigning against the weaker of your two opponents in a generalized three-candidate model, which includes the present model as a special case.

11. However, we do not think we can derive this objective function from more fundamental assumptions on the error terms, as can be done with the payoff functions in two-candidate competition (see n. 8).

12. In addition to the vast body of work on electoral competition in a single or multidimensional issue space springing from the work of Anthony Downs (1957; see reviews in Enelow and Hinich 1984, 1990 and Grofman 1993), there is work specifically on how decisions about campaign contributions are made (e.g., Ben Zion and Eytan 1975; Bental and Ben Zion 1975; Crain and Tollison 1976); on the optimal allocation of campaign appearances and campaign funds in space and time (e.g., Aldrich 1980; Bartels 1988; Brams and Davis, 1974; Colantoni, Levesque, and Ordeshook 1975; Lave and March 1975); and on the consequence of campaign finance regulation (e.g., Aranson and Hinich 1979). Two more recent contributions are Thomas 1990 and Hammond and Humes 1993. Thomas posits that candidates do cost–benefit calculations of various types of campaigning; and in the Hammond and Humes model, each candidate emphasize the issues in which he or she does best.

13. Harrington and Hess (1993) distinguish between policy and nonpolicy dimensions, with campaigning taking place along the policy dimension. Positive campaigning influences the candidate's own position—negative campaigning, the opponent's position. One main finding of Harrington and Hess, complementing ours on the effect of candidates' voter support, is that candidates weaker in the nonpolicy dimension engage in more negative campaigning than their opponents.

14. In the original version of the truel, each duelist has a certain number of bullets and a certain probability of hitting his target, and only one opponent can be fired at in any round. At each round, each duelist simultaneously chooses whether or not to fire and, if so, on whom to fire. (For several other variations, see Dresher 1981.) With respect to truels, the questions that can be asked are (1) What is each duelist's optimal strategy, that is, when would they shoot and at whom? and (2) If each duelist follows an optimal strategy, how does the probability of survival depend upon the duelists' relative shooting accuracies? In particular, is the most accurate duelist most likely to survive? See Shubik 1954.

15. We believe that treating the 1992 contest as a plurality election is a reasonable approximation. While the actual mode of selection of the president is by a complex process that includes the weighted voting rule that determines the electoral college vote and the rule for how to select a president if no candidate receives a majority of the electoral college vote, voter attention in 1992 was focused heavily on the national popular vote.

16. In general, in 1992, neither major party candidate sought to distinguish their policy stands from those of Perot except on peripheral issues such as the gasoline tax. Journalists asserted that the reason for this reluctance to distinguish their own issue positions from those of Perot was that neither Clinton nor Bush wanted to alienate Perot supporters, whom they hoped to eventually woo to their side. However, this does not satisfactorily explain why there was repeatedly expressed policy divergence between the front-runners Clinton and Bush. We believe that a sensible rational choice story can be told about relative policy position and divergence in three-candidate contests, but this is a story that we cannot pursue here.

17. Impressionistically, it seems to us that voter backlash against negative campaigning was not constant but increased in the last phases of the campaign, as signaled by the question asked by a member of the audience early in the second debate that was prefaced by a reprimand to the candidates for having engaged in too much negative campaigning.

18. See discussion of this point in Kern 1989 and Johnson-Cartee and Copeland 1991.

19. For the importance of the latter part, see Ansolabehere et al. (n.d.).

References

Aldrich, John H. 1980. *Before the Convention: Strategies and Choices in Presidential Nomination Campaigns.* Chicago: University of Chicago Press.

Ansolabehere, Stephen, and Shanto Iyengar. 1991. "The Electoral Effects of Issues and Attacks in Campaign Advertising." Presented at the annual meeting of the American Political Science Association, Washington.

Ansolabehere, Stephen, and Shanto Iyengar. 1993. "How Persuasive Are Campaign Advertisements?" University of California, Los Angeles. Mimeo.

Ansolabehere, Stephen, Shanto Iyengar, Adam Simon, and Nicholas Valentino. 1994. "Does Attack Advertising Demobilize the Electorate?" *American Political Science Review* 88: 829–838.

Aranson, Peter H., and Melvin J. Hinich. 1979. "Some Aspects of the Political Economy of Election Campaign Contribution Laws." *Public Choice* 34:435–61.

Aranson, Peter H., Melvin J. Hinich, and Peter C. Ordeshook. 1974. "Election Goals and Strategies: Equivalent and Nonequivalent Objectives." *American Political Science Review* 68:135–52.

Bartels, Larry M. 1988. *Presidential Primaries and the Dynamics of Public Choice.* Princeton: Princeton University Press.

Bartels, Larry M. 1993. "Messages Received: The Political

Impact of Media Exposure." *American Political Science Review* 87:1–24.

Beiler, David. 1992. "Patriotic Pandering: Our Electoral Process is in a Crisis—as Always." *Campaign Issues*, November, pp. 27-30.

Ben Zion, U., and Z. Eytan, 1975. "On Money, Votes, and Policy in a Democratic Society." *Public Choice* 28:400–405.

Brams, Steven J., and Morton D. Davis. 1975. "The 3/2's Rule in Presidential Campaigning." *American Political Science Review* 68:113–34.

Colantoni, Claude S., T. J. Levesque, and P. C. Ordeshook. 1975. "Campaign Resource Allocations under the Electoral College." *American Political Science Review* 69:141–54.

Coughlin, Peter. 1986. "Probabilistic Voting Models." In *Encyclopedia of Statistical Sciences*, vol. 7, ed. Samuel Kotz and Norman L. Johnson. New York: Wiley & Sons.

Crain, W. M., and R. Tollison. 1976. "Campaign Expenditures and Political Competition." *Journal of Law and Economics* 19:177–88.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper.

Dresher, Melvin. 1981. *The Mathematics of Games of Strategy*. New York: Dover.

Ehrenhalt, Alan A. 1985. "Technology, Strategy Bring New Campaign Era." *Congressional Quarterly Weekly Report* 55: 2559–65.

Enelow, James M., and Melvin J. Hinich. 1985. *The Spatial Theory of Voting: An Introduction*. New York: Cambridge University Press.

Enelow, James M., and Melvin J. Hinich. 1990. *Advances in the Spatial Theory of Voting*. New York: Cambridge University Press.

Franklin, Charles. 1991. "Eschewing Objection? Campaigning and the Perception of U.S. Senate Candidates." *American Political Science Review* 85:1193–1214.

Garramone, Gina M. 1984. "Voter Response to Negative Political Ads." *Journalism Quarterly* 61:250–59.

Garramone, Gina M. 1985. "Effects of Negative Political Advertising: The Roles of Sponsor and Rebuttal." *Journal of Broadcasting and Electronic Media* 29:147–59.

Grofman, Bernard, ed. 1993. *Information, Participation, and Choice: "An Economic Theory of Democracy" in Perspective*. Ann Arbor: University of Michigan Press.

Grofman, Bernard, and Julie Withers. 1993. "Information Pooling Models of Electoral Politics." In *Information, Participation, and Choice*, ed. Bernard Grofman. Ann Arbor: University of Michigan Press.

Hammond, Thomas, and Brian Humes. 1993. "'What This Campaign Is All About Is . . .': A Rational Choice Alternative to the Downsian Model of Elections." In *Information, Participation, and Choice*, ed. Bernard Grofman. Ann Arbor: University of Michigan Press.

Harrington, Joseph Jr., and Gregory D. Hess. 1993. "A Theory of Campaign Strategy." Johns Hopkins University. Mimeo.

Hinich, Melvin J., and Peter C. Ordeshook. 1970. "Plurality Maximization Versus Vote Maximization." *American Political Science Review* 64:772–91.

Hirshleifer, Jack. 1989. "Conflict and Rent-seeking Success Functions: Ratio Versus Difference Models of Relative Success." *Public Choice* 63:101–12.

Iyengar, Shanto, and Donald Kinder. 1978. *News That Matters*. Chicago: University of Chicago Press.

Iyengar, Shanto, M. D. Peters, and Donald Kinder. 1982. "Experimental Demonstrations of the 'Not So Minimal' Consequences of Television News Programs." *American Political Science Review* 76:848–58.

Johnson-Cartee, Karen S., and Gary A. Copeland. 1989. "Southern Voters' Reaction to Negative Political Ads in the 1986 Election." *Journalism Quarterly* 66:888–93, 986.

Johnson-Cartee, Karen S., and Gary A. Copeland. 1991. *Negative Political Advertising: Coming of Age*. Hillsdale, NJ: Lawrence Erlbaum.

Kaid, Lynda Lee, and D. K. Davidson. 1986. "Elements of Videostyle: Candidate Presentation through Television Advertising." In *New Perspectives on Political Advertising*, ed. L. L. Kaid, Dan Nimmo, and K. R. Sanders, Carbondale: Southern Illinois University Press.

Kellerman, K. 1984. "The Negativity Effect and Its Implications for Initial Interaction." *Communication Monographs* 51:37–55.

Kern, Montague. 1989. *Thirty-second Politics: Political Advertising in the Eighties*. New York: Praeger.

Lau, Richard R. 1985. "Two Explanations for Negativity Effects in Political Behavior." *American Journal of Political Science* 30:119–38.

Lave, Charles, and James March. 1975. *Introduction to Models in the Social Sciences*. New York: Harper & Row.

Luce, R. Duncan, and Patrick Suppes. 1965. "Preferences, Utility, and Subjective Probability." In *Handbook of Mathematical Psychology*, vol. 3, ed. R. D. Luce, R. R. Bush, and E. Galanter. New York: Wiley.

Merritt, S. 1984. "Negative Political Advertising: Some Empirical Findings." *Journal of Advertising* 13:27–38.

Patterson, Thomas, and Robert McClure. 1973. *Political Advertising: Voter Reaction to Televised Political Commercials*. Princeton: Citizens Research.

Pfau, Michael, and Henry C. Kenski. 1990. *Attack Politics: Strategy and Defence*. New York: Praeger.

Popkin, Samuel. 1991. *The Reasoning Voter: Communication and Persuasion in Presidential Campaigns*. Chicago: University of Chicago Press.

Roddy, B. L., and Gina M. Garramone. 1988. "Appeals and Strategies of Negative Political Advertising." *Journal of Broadcasting and Electronic Media* 32:415–27.

Rosenberg, Shawn, Lisa Bohan, Patrick McCafferty, and Kevin Harris. 1986. "The Image and the Vote: The Effect of Candidate Presentation on Voter Preference." *American Journal of Political Science* 31:108–25.

Shubik, Martin. 1954. "Does the Fittest Necessarily Survive?" In *Readings in Game Theory and Related Behavior*, ed. Martin Shubik. New York: Doubleday.

Skaperdas, Stergios. 1992. "Cooperation, Conflict, and Power in the Absence of Property Rights." *American Economic Review* 82:720–39.

Skaperdas, Stergios. 1994. "Contest Success Functions." University of California, Irvine. Mimeo.

Suppes, Patrick, David H. Krantz, R. Duncan Luce, and Amos Tversky. 1989. *Foundations of Measurement*. Vol. 2. San Diego: Academic.

Surlin, Stuart H., and Thomas F. Gordon. 1977. "How Values Affect Attitudes toward Direct Reference Political Advertising." *Journalism Quarterly* 54:89–98.

Tarrance, Lance. 1982. *Negative Campaigns and Negative Votes: The 1980 Elections*. Washington: Free Congress Research and Education Foundation.

Thomas, Scott. 1990. "Negative Advertising Theory of Campaign Expenditures." In *Predicting Politics*, ed. W. Mark Crain and Robert D. Tollison. Ann Arbor: University of Michigan Press.

Tullock, Gordon. 1980. "Efficient Rent Seeking." In *Toward a Theory of the Rent-seeking Society*, ed. J. M. Buchanan, R. D. Tollison and G. Tullock. College Station: Texas A&M University Press.

Weatherford, H. Stephen. 1993. "'An Economic Theory of Democracy' as a Theory of Policy." In *Information, Participation, and Choice*, ed. Bernard Grofman. Ann Arbor: University of Michigan Press.

Wood, Gordon S. 1978. "The Democratization of Mind in the American Revolution." In *The Moral Foundation of the American Republic*, ed. R. Horowitz. Charlottesville: University Press of Virginia.

Young, Michael L. 1987. *American Dictionary of Campaigns and Elections*. Lanham, MD: Hamilton.

Zaller, John. 1992. *The Origins and Nature of Mass Opinion*. New York: Cambridge University Press.

Stergios Skaperdas is Assistant Professor of Economics and Bernard Grofman is Professor of Political Science, University of California—Irvine, Irvine, CA 92717.

American Political Science Review Vol. 89, No. 1 March 1995

POLITICAL CONTROL VERSUS EXPERTISE: CONGRESSIONAL CHOICES ABOUT ADMINISTRATIVE PROCEDURES

KATHLEEN BAWN *University of California, Los Angeles*

Congressional choices about administrative procedures affect an agency's political responsiveness and the technical accuracy of its decisions. Legislators would like to design procedures so that agencies make technically sound decisions and balance the needs of competing interests in the way intended. In practice, agency procedures designed to promote technical competence often allow for political drift, and those that promote political control provide little new technical information about the consequences of policy decisions. The trade-off between technical competence and political control is captured in a model of a legislative coalition's decision about agency procedures. The choice variables are the agency's expected preferences and independence. Depending on exogenous levels of technical and political uncertainty, optimal agency procedures can maximize technical competence, maximize political control, or achieve a combination of the two.

By delegating policy decisions to the bureaucracy, Congress creates a control problem for itself. Of course, Congress retains influence over bureaucratic decisions, but this influence can be offset by presidential opposition, court decisions, or the actions of agency personnel. Congress employs a variety of strategies to mitigate the principal-agent problem between itself and the bureaucracy, but the fact remains that the problem is created by the congressional decision to delegate. Even when Congress employs every possible strategy to control agency decisions, the possibility of "bureaucratic drift" (that is, the possibility that agency decisions do not serve intended purposes) remains.

Why does Congress create this problem of political control? One explanation is what Kiewiet and McCubbins (1991) call the "abdication hypothesis." This hypothesis implies that there is no principal-agent problem because the principal (Congress) does not care much about the outcome of the agency's policy decision. For example, if delegation allows members of Congress to claim credit for the benefits while avoiding blame for costs, members may not have strong preferences over what particular policy the agency chooses (Fiorina 1982).

The abdication hypothesis is not consistent with efforts made by Congress to control the bureaucracy. We observe overt oversight of agencies (Aberbach 1990), bureaucratic reactions to changing congressional preferences (Weingast and Moran 1983), and sophisticated design of administrative procedures to control agencies (Bawn 1992; Gilligan, Marshall and Weingast 1989; McCubbins, Noll and Weingast 1987, 1989; McCubbins and Schwartz 1984). Congress appears to care about the outcomes of at least some delegated policy decisions.

A different explanation for why Congress creates principal-agent problems and then devotes effort to solving them emphasizes uncertainty about policy consequences. Delegation allows better information to be obtained about the consequences of alternative policies (Gilligan and Krehbiel 1987; Kiewiet and McCubbins 1991; McCubbins 1985). How many fewer accidents will result from product standard A than standard B? How much will emissions standard C improve air quality? Will jobs be lost? If legislators want policy decisions to reflect these concerns, they will delegate when the gains from agency expertise outweigh losses from potential agency drift.

As McCubbins (1985) argued, delegation is not an all-or-nothing decision. When Congress delegates a policy decision, it chooses the procedures that determine how the agency will make the policy choice. Procedures determine who participates in agency decisions, what information is used, and how agency decisions can be challenged. I shall use a broad definition of "procedures," encompassing such structural issues as which agency makes the decision, how the agency is organized, what qualifications are required for key personnel, and how the agency relates to the rest of the bureaucracy. Legislative coalitions can use procedures to enfranchise some groups and exclude others. By specifying who has access to the agency, procedures reduce uncertainty about the agency's decisions and effectively limit agency independence.

I shall focus on agency independence as a choice made by Congress. For this reason, I define agency independence from Congress's point of view, rather than the agency's. An agency is more independent, the less its decisions are determined in advance by Congress. Consider, for example, an agency structured so that it makes policy decisions the same way Congress would but without overt intervention. For my purposes here, this agency is not very independent, even though it may seem unconstrained ex post. My definition of *independence* is thus close to what McCubbins (1985) called "effective discretion." *Independence* refers to an agency's potential to drift from legislative intent ex ante, not its realized influence on policy ex post (as in, e.g., Calvert, McCubbins and Weingast 1989).

Procedures can reduce independence by making an agency rely on private interests for information, imposing tight deadlines, or making certain types of decisions difficult to promulgate (McCubbins, Noll, and Weingast 1987, 1989; Moe 1989).

Procedures also affect an agency's ability to gather information about the consequences of the policy decisions it makes. To cite a famous example, procedures in the 1970 Clean Air Act Amendments instructed the Environmental Protection Agency (EPA) to set national air quality standards without regard to cost information. This procedural requirement diminished the EPA's ability to predict cost-associated components of policy outcomes.

Ideally, legislators would like to design administrative procedures so that the agency gathers objective and complete information about policy consequences and then uses the information exactly the way the legislators themselves would. The problem is that administrative procedures designed to prevent bureaucratic drift also limit the agency's ability to research policy consequences or to make decisions that reflect its expertise (Ackerman and Hassler 1981; Noll 1971). Conversely, the isolation and independence necessary for objective expertise make an agency's political responsiveness highly sensitive to the personal preferences of its staff, making agency decisions hard to predict and hard to control (Noll 1983).

My central thesis will be that the degree of agency independence on any particular policy reflects the legislature's willingness to trade uncertainty about policy consequences for uncertainty about agency behavior. I model the choice of agency procedures by a coalition in Congress. Choosing agency procedures amounts to choosing three things. First, procedures determine which groups in society the agency is most responsive to. In this sense, procedures establish the agency's effective preferences over policy outcomes. Procedures also determine the degree of uncertainty about agency preferences (the degree of political control) and uncertainty about policy consequences (expertise). The model captures these three aspects of procedures as (1) the mean of the distribution from which the agency's ideal point is chosen, (2) the variance of this distribution, and (3) the variance of the agency's prediction of policy consequences. The model predicts that the legislative coalition will always choose procedures to give an agency's expected ideal point identical to its own. However, the choice between control and expertise will vary depending on technical features of the policy and the political environment.

MODELING AGENCY INDEPENDENCE

I bridge two approaches to studying congressional influence on agency decision-making. The first emphasizes the ability of Congress to "stack the deck" in agency policy-making. The second approach is game theoretic and emphasizes information asymmetrics.

The first approach argues that procedural decisions by Congress critically affect subsequent policy decisions by the agency. McCubbins, Noll, and Weingast (1987, 1989, 1990a–b) claim that administrative procedures are chosen on the basis of strategic political considerations.[1] Administrative procedures are designed to "stack the deck" in favor of groups that the bill's support coalition wants to help. This means that procedures lead the agency to respond to competing interests in the same way as would the enacting coalition in Congress. McCubbins, Noll and Weingast infer that procedures are designed to create an agency whose preferences over policy outcomes—preferences over who is ultimately helped and hurt by the policy—are the same as the enacting coalition's.

The key implication of McCubbins, Noll, and Weingast's argument is that agency preferences are endogenous, a function of procedural choices made by Congress. Agency preferences are induced by institutional constraints (administrative procedures), just as the preferences of legislators are induced by the definition of congressional districts and other aspects of electoral institutions.

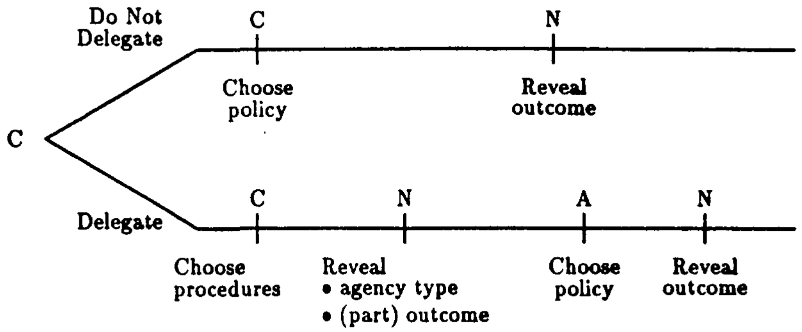
Procedures control agencies more effectively in some circumstances than others, however. Hill and Brazier (1991) describe various ways that procedures fail—for example, because of an uncooperative judiciary. Arnold (1987) argues that the effect of policy-specific procedures can be changed by laws like the Freedom of Information Act and the National Environmental Policy Act, which affect all agency decisions. Horn and Shepsle (1989) point out that some procedures run the risk of backfiring by isolating the agency from friendly, as well as hostile, interests. My model incorporates the notion that the efficacy of procedural control is conditional on many facets of the political environment.

The relative importance of deck stacking in the design of procedures is subject to debate. Robinson (1989) argues that deck stacking does not occur often enough to explain observed choices of procedures. Arnold (1987) questions whether procedural control is as effective as more "traditional" means, including oversight hearings and threats to withhold appropriations. On the other hand, Moe (1990) argues that political motivation in the choice of agency procedures—and the choice of institutions in general—is seriously underestimated. This question is clearly an empirical one. One goal of my formal model is to define the testable implications of the deck-stacking theory.

The second approach to understanding agency independence involves game theoretic models of particular procedures. Building on game theoretic models of delegation within Congress (e.g., Gilligan and Krehbiel 1987), this literature explicitly models administrative procedures in games of incomplete information. Important papers in this newer tradition include Spiller and Urbiztondo (1991) on the hearings requirements of the Administrative Procedures Act, Martin (1992) on legislative vetoes,[2] Epstein and O'Halloran (1993) on the scope of agency discretion,

FIGURE 1

Sequence of Decisions in Agency Policymaking



and Lupia and McCubbins (1994) on interest group participation.

In contrast to the deck-stacking literature, game-theoretic models assume that agency preferences are given exogenously. The claim that agency preferences are endogenously induced by procedures is difficult to incorporate into a game-theoretic model. The choice I want to explain is actually a decision (of the legislature) rather than a game (between Congress and an agency). Even though there are technically two decision makers, the institutions that determine agency preferences are chosen within the model. Sometimes, of course, legislative coalitions do choose to delegate to an existing agency with existing preferences, but this is one choice from a set of many procedural options. My model assumes that the agency's future strategic opportunities are anticipated by Congress. This allows me to focus solely on the decision of the legislative coalition.

A MODEL OF THE CHOICE OF ADMINISTRATIVE PROCEDURES

Figure 1 illustrates the sequence of decisions in the choice of administrative procedures by a legislative coalition that cares about outcomes. The coalition (*C*) decides whether to delegate the policy choice. If not, the coalition makes the policy choice itself, based on incomplete information, and Nature (*N*) reveals the outcome. If delegation is chosen, the coalition chooses administrative procedures. Nature chooses the agency's preferences from a distribution determined by procedures. The agency (*A*) chooses a policy based on better, but still incomplete, information. First, I shall discuss the logic behind the formal model. The mathematical assumptions are stated explicitly at the end.

The Coalition Chooses Procedures

The legislative coalition consists of legislators and (normally) the president. The coalition members care about the consequences of government policies: public health, quality of life, industry profits, level of employment. Their preferences can thus be characterized in terms of ideal points in outcome space, which here has one dimension. In reality, the coalition members' ideal points are likely to differ because they represent different constituencies. Nonetheless, I treat the coalition as a single, unified actor. This simplification ignores disagreement within the coalition (McCubbins 1985; Moe 1989) and the institutions that structure legislative bargaining (McCubbins, Noll, and Weingast 1989). However, it allows me to focus on the trade-off between expertise and control. I shall return to the important issue of intracoalition conflict in the final section.

I make the conventional assumption that the coalition's preferences are quadratic, characterized by an ideal point, c^*. The coalition's preference function is the standard $U_C(z) = -(z - c^*)^2$, where z is the outcome of the chosen policy.

The policy outcome (z) is a function of the policy (p) and unknown states of the world, represented by a random variable (ε). The legislative coalition's estimate of policy consequences is $\tilde{z}_C$. Tildes (~) denote estimates throughout the paper. The subscript C identifies $\tilde{z}_C$ as the estimate of the coalition, as opposed to the agency. The coalition's estimate has an independent error term, ε, with mean zero, so that $z(p) = \tilde{z}_C(p) + \varepsilon$. The variance of the error in the legislative coalition's estimate is a known exogenous parameter, v_ε. I refer to this initial uncertainty about policy consequences as *intrinsic technical uncertainty*—*technical* because it is due to incomplete knowledge of natural processes (e.g., dispersion patterns from a smokestack) and economic responses (relocation of industry). The word *intrinsic* distinguishes this initial level of technical uncertainty from lower levels that may be obtained later through agency expertise.

When delegating, the coalition chooses administrative procedures based on how they affect the agency's policy decisions. The link between procedures and agency choices is discussed next.

The Agency Chooses Policy

The specification of agency preferences is based on the *external signals model*, developed by Noll (1985) and used in several empirical studies of agency rule making (Hamilton and Schroeder 1994; Magat, Krupnick, and Harrington 1986; Olson 1994). The behavioral assumption is that the agency reacts to feedback from a variety of external sources, including interest groups, private citizens, and elected officials. Agency decisions typically provoke positive feedback from some groups and negative feedback from others. The agency is assumed to maximize net positive feedback.

The external signals model encompasses many possible theories of agency behavior. The weight given to particular sources of feedback presumably varies from agency to agency. Depending on these weights, the model can incorporate capture theory (the strength of the signal from the regulated industry overwhelms all others), so-called congressional dominance theories (the strongest signal comes from the subcommittee overseeing the agency), and theories based on personal or professional ideology (the signal takes the form of the professional reputation of agency staff). This versatility accommodates Wilson's (1980) argument that any theory of agency behavior must take account of the observed differences among agencies. The external signals framework is also consistent with the idea that agencies avoid effort, because any source of feedback can impose effort on the agency, for example, by conducting oversight and audits or by bringing cases for judicial review of agency decisions.[3]

I assume that the agency's feedback sources react to outcomes and that external signals have diminishing marginal impact on the agency. These assumptions are sufficient to give the agency an objective function characterized by an ideal point in outcome space, of the same basic form as the legislative coalition's.

The external signals model offers a natural way to model procedures. The deck-stacking literature argues that administrative procedures affect policy outcomes by allowing some groups easier access to the agency's decision process than others (McCubbins and Schwartz 1984; McCubbins, Noll, and Weingast 1987). In the language of the external signals model, procedures determine the relative strengths of signals received from different groups.

The location of an agency's ideal point is thus endogenous, determined by the influence that procedures give to different groups. Members of the legislative coalition understand how specific procedural choices amplify the signals of some interests relative to others. They also understand the type of signals that groups will send in response to particular outcomes. The coalition chooses procedures so as to create an agency with the desired induced ideal point.

Of course, the enacting coalition cannot perfectly anticipate the precise impact of a particular procedure. For example, procedures can have unanticipated consequences because legislative coalitions miscalculate the strength and goals of interest groups. Consider the procedural requirement that an agency take compliance costs into account when deciding an emissions standard. This requirement makes the agency more likely to be more responsive to industry. But this effect is not guaranteed. Unexpected behavior by agency staff or innovative reactions by environmental groups can offset anticipated effects of administrative procedures.

Moreover, as Horn and Shepsle (1989) argue, some procedures have relatively uncertain impact on agency decisions. Consider again the example of costs in emissions standards. The likely effect—an increase in the agency's responsiveness to industry—becomes more certain as the agency is given fewer resources with which to evaluate cost data submitted by industry. If the enacting coalition wants to decrease uncertainty about the effect of this procedural requirement, it can reduce agency independence by establishing shorter deadlines and less access to outside expertise. The agency will be less competent to evaluate industry data objectively and thus more likely to accept it at face value. Environmental groups will have less of an opportunity to respond. In this example, shortening deadlines has the effect of reducing uncertainty about the agency's induced preferences.

Procedural Uncertainty and Agency Independence

The inherent uncertainty in procedural control means that the legislative coalition chooses not the agency's actual ideal point but, rather, the distribution from which the ideal point is drawn. Procedures determine both the mean and the variance of this distribution. Procedures are thus captured in the model by two choice variables, m and x. Variable m represents aspects of procedures that affect the expected value of the agency's ideal point, and variable x, aspects that affect its variance.

As the compliance-cost example illustrates, the choice of procedures is really a set of many interlocking choices. In principal, the effect of each procedural requirement (data requirements, deadlines, veto points) on outcomes could be explicitly modeled in a giant game with many parameters. The equilibria of this game could then be studied to predict how the legislative coalition would choose procedures under all combinations of parameter values. In practice, this approach is too complex to be tractable. My focus is on the total impact of all procedural decisions on policy outcomes. For this reason, I represent administrative procedures as a simple, two-dimensional choice variable.

The agency's true ideal point, a^*, can be written as the combination of the predicted effects of administrative procedures, $\tilde{a}_C$, and a random error term τ: $a^*(m) = \tilde{a}_C(m) + \tau$. Choice variable m represents the relative weight the agency is expected to give to those who pay the costs of regulatory decisions over those who experience the benefits.

Random variable τ captures unforeseen influences on the agency's induced preferences. It has mean zero and a known variance, v_τ. The variance of τ represents the coalition's uncertainty about the political impact of administrative procedures. Hence, I call v_τ *procedural uncertainty*.

Procedural uncertainty depends partly on intrinsic factors beyond the control of the legislative coalition and partly on chosen procedures. Thus, v_τ is a function of positive exogenous parameters, s and v_0, and of a choice variable, x: $v_\tau(x) = v_0(1 - e^{-sx})$. Variable x represents all aspects of administrative procedures that affect ex ante uncertainty about the agency's induced preferences. From the point of view of Congress, x is agency independence.

This functional form for v_τ is not merely convenient: it captures four important features of reality. First, procedural uncertainty is zero when the agency has no independence ($x = 0$). It is possible to structure an agency to respond with certainty exactly as the coalition wants, that is, to set $v_\tau = 0$.

Second, there is a finite limit to uncertainty about an agency subject to absolutely no procedural controls. This limit is given by an exogenous parameter, v_0, which takes different values depending on the nature of the policy and the institutional environment. I refer to v_0 as *intrinsic procedural uncertainty*. This parameter is the level of procedural uncertainty that would occur if the legislative coalition made no attempt to control the agency through procedures. Intrinsic procedural uncertainty is higher, for example, in periods of divided government, because the agency may be subject to conflicting pressures from different elected officials. The Supreme Court's *Chevron* decision of 1984 provides a second example of a change in v_0. By ruling that agency decisions should be upheld unless in clear conflict with written law, the Court arguably raised v_0 for all agencies.

Third, the rate at which an increase in agency independence increases procedural uncertainty can also vary across policy areas and through time. For example, procedures to constrain agency independence work more effectively when groups affected by the policy are predictable and well established, relative to when interests are newly organized and of uncertain strength. The rate at which independence affects uncertainty is given by another exogenous parameter, s.

Finally, this specification gives $v''_\tau(x) < 0$, implying that the marginal loss of political control decreases as independence increases. Additional independence permits additional agency drift, but at a decreasing rate.

Technical Uncertainty and Independence

Like the legislative coalition, the agency does not completely know policy consequences in advance, but it has an unbiased estimate ($\tilde{z}_A$), such that $z(p) = \tilde{z}_A(p) + \delta$. The random variable δ is the error in the agency's estimate of policy consequences. Like ε, the error in the legislative coalition's estimate, δ has mean zero and a known variance, v_δ. However, δ and ε do not necessarily have the same variance. The agency's estimate of policy consequences will be at least as good as the coalition's and possibly better, so that $v_\delta \leq v_\varepsilon$.

Increasing independence increases the agency's ability to incorporate knowledge of policy consequences into its decisions. The level of independence determines how much more accurate the agency's estimate is relative to the coalition's. The following specification of v_δ captures this relationship between the coalition's intrinsic technical uncertainty (v_ε), its choice of agency independence (x), and the agency's resulting technical uncertainty: $v_\delta = v_\varepsilon e^{-rx}$.

As with procedural uncertainty, this functional form was chosen because it reflects important aspects of reality. An agency with no independence ($x = 0$) has information about policy consequences no better than the legislative coalition's ($v_\delta = v_\varepsilon$). As independence increases, the agency's uncertainty about policy consequences decreases but is never completely eliminated. The rate at which independence improves agency information depends on an exogenous parameter, r, presumed to vary across policy areas. The rate is likely to be higher, for example, when policy consequences can be naturally quantified than when consequences involve less tangible things like quality of life.

The model treats the agency's level of expertise as a continuous variable. This approach differs from most game-theoretic models in which the agency learns policy outcomes with certainty, and only the legislative coalition is uncertain. Assuming that the agency has no uncertainty is unproblematic when focusing on information asymmetries and particular procedures. My focus, however, is on the broader trade-off between control and expertise in the complete package of procedures. To capture this trade-off effectively, I cannot treat expertise as an all-or-nothing variable. Modeling uncertainty (both technical and procedural) as the variance of a distribution gives me the required continuous specification.[4]

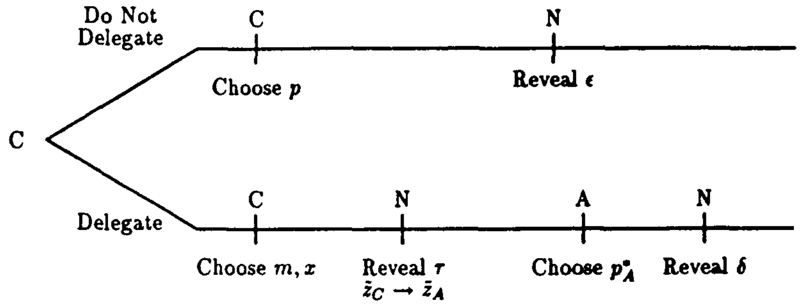
The assumption that increasing agency independence increases technical knowledge is consistent with previous findings. Baron and Myerson (1982) showed theoretically that when an agency depends on interested parties for information, decisions systematically favor the source of information, and outcome forecasts are less certain. McCubbins, Noll, and Weingast (1987) observed empirically that legislative coalitions attempt to control agencies by requiring them to use information only available from a group affected by the policy decision. The assumption is also consistent with results about delegation within Congress. Gilligan and Krehbiel (1987) found that when committees are given more independence (via closed rules), they have greater incentive to undertake the effort necessary to find out policy consequences. My model's agency is analogous to Gilligan and Krehbiel's committee in that it is the recipient of delegated authority and has the opportunity to improve knowledge of policy consequences.

A Summary of the Formal Model

The model is summarized by the time line in Figure 2. The sequence of decisions is the same as in Figure 1, and variables names replace the verbal descriptions. The focus is on the first two decisions: whether to

FIGURE 2

Sequence of Decisions in Model of Delegation



delegate and what procedures to choose. Procedures consist of the expected responsiveness (*m*) and agency independence (*x*). While variable *m* is included for logical completeness, I am primarily concerned with agency independence, *x*. Independence simultaneously determines the variance of the agency's induced preferences and the variance of its estimate of policy consequences. Independence is thus the lever through which the trade-off between expertise (reduction of technical uncertainty) and control (reduction of procedural uncertainty) is made.[5]

After the coalition chooses administrative procedures, Nature reveals τ, the unpredictable effects of procedures, and the true preferences of the agency (a^*) become known. At this stage, some technical uncertainty is resolved as the agency forms its estimate of policy consequences, $\tilde{z}_A$, which has a lower variance than the coalition's. The agency then chooses policy p^*_A on the basis of its induced preferences (a^*) and beliefs ($\tilde{z}_A$).

The following assumptions thus constitute the formal model of choice of administrative procedures:

ASSUMPTION 1. (Coalition's Preferences). *The legislative coalition maximizes* $U_C(z) = -(z - c^*)^2$.

ASSUMPTION 2. (Agency's Preferences). *The agency maximizes* $U_A(z) = -(z - a^*)^2$.

ASSUMPTION 3. (Coalition's Technical Uncertainty). *The legislative coalition's best estimate of policy consequences is* $\tilde{z}_C(p)$, *where* $z(p) = \tilde{z}_C(p) + \varepsilon$. *Random variable* ε *has mean zero and exogenously determined variance* v_ε.

ASSUMPTION 4. (Agency's Technical Uncertainty). *The agency's best estimate of policy consequences is* $\tilde{z}_A(p)$, *where* $z(p) = \tilde{z}_A(p) + \delta$. *Random variable* δ *has mean zero and variance given by* $v_\delta(x) = v_\varepsilon e^{-rx}$, *where* x *is a choice of the legislative coalition and* r *is a positive exogenous parameter.*

ASSUMPTION 5. (Coalition's Procedural Uncertainty). *The legislative coalition's best estimate of the political effects of procedures* (m) *is* $\tilde{a}_C(m)$, *where* $a(m) = \tilde{a}_C(m) + \tau$ *and* m *is a choice of the legislative coalition. Random variable* τ *has mean zero and variance given by* $v_\tau(x) = v_0(1 - e^{-sx})$, *where* s *and* v_0 *are positive exogenous parameters.*

Assumptions 1 and 2 define the preferences that determine behavior in the model. Assumptions 3–5 describe the information upon which the decisions are made. The following section describes the choices resulting from these preferences and information.

OPTIMAL CHOICE OF AGENCY PROCEDURES

Because the coalition's preferences are defined over outcomes, it makes procedural decisions by anticipating probable effects on outcomes. This implies that the decision sequence in Figure 1 should be solved backward: the legislative coalition calculates its expected payoff by estimating the consequences of policies that different procedures promote.

The model is designed to show how procedural choices differ from one policy area to another, depending on intrinsic technical and procedural uncertainty and on the rate at which agency independence affects these problems. The optimal level of agency independence is thus a function of exogenous parameters, r, s, v_ε and v_0. The interpretations of the exogenous parameters and choice variables are summarized as follow:

Choice Variables

p	policy
m	one dimension of procedures, determines agency's expected ideal point
x	other dimension of procedures, agency independence

Exogenous Parameters

r	rate at which agency independence improves technical knowledge
s	rate at which agency independence permits drift
v_ε	intrinsic technical uncertainty
v_0	intrinsic procedural uncertainty

Random Variables

ε	error in coalition's estimate of policy consequences
δ	error in agency's estimate of policy consequences
τ	error in coalition's estimate of the agency's induced ideal point

Endogenously Determined Variables

$z(p, \delta)$	outcome of policy choice
$a^*(m, \tau)$	agency's ideal outcome
$v_\tau(x)$	variance of coalition's estimate of agency ideal piont
$v_\delta(x)$	variance of agency's estimate of policy consequences

The optimal choices for different exogenous scenarios are discussed below. Proofs appear in the Appendix.

RESULT 1. *Optimal administrative procedures give an expected agency ideal point equal to the coalition's ideal point.*

Result 1 is the formal equivalent of the argument made by McCubbins, Noll, and Weingast (1989) that administrative procedures will mirror politics within the enacting coalition. A new agency's relationship to existing parts of the bureaucracy, its access to information, and its permeability by interest groups will combine to make it respond, in expectation, the way that the legislative coalition would have.

RESULT 2. *The coalition's expected payoff from delegating to an agency with no independence is the same as from legislating the policy itself.*

Result 2 is more of a statement about the logical structure of the model than a prediction. It simplifies the analysis, incorporating the dichotomous choice (delegate or not) into the continuous choice (how much independent behavior to allow). In this model, political benefits depend only on policy consequences. There is no blame shifting. The electorate and interest groups hold legislators responsible for the actions of the agencies that they create. The subsequent results show that blame-shifting concerns are not necessary for legislative preferences over agency independence to differ across policy areas.

The remaining results characterize the coalition's choice of agency independence: its choice of expertise versus control.

RESULT 3. *When* $r > s$ *and* $\frac{v_\varepsilon}{v_0} \geq \frac{s}{r}$*, the optimal level of agency independence is*

$$x^* = \frac{\ln \frac{r v_\varepsilon}{s v_0}}{r - s}. \qquad (1)$$

Result 3 states the conditions for which a compromise between agency expertise and control is optimal. There are two requirements for such a compromise to be chosen. First, the rate at which agency independence improves knowledge must be higher than the rate at which it damages control, that is, $r > s$. The condition $r > s$ is necessary for there to be decreasing marginal returns to agency independence. Specifically, when $r > s$, decreasing marginal benefits of independence (improved knowledge of policy consequences) dominate the effect of decreasing marginal costs (loss of agency control). This results in overall decreasing returns to scale, and a finite solution for x. When $r \leq s$, the effect of decreasing marginal costs dominates, resulting in increasing returns to scale and corner solutions, as detailed in results 4 and 5. The second requirement for the optimal administrative procedures to strike a balance between control and expertise is that technical uncertainty must not be a trivial problem relative to procedural uncertainty

$$\left(\frac{v_\varepsilon}{v_0} \geq \frac{s}{r}\right).$$

Given the first requirement ($r > s$), it is sufficient but not necessary that $v_\varepsilon > v_0$.

RESULT 4. *When* $v_\varepsilon < v_0$ *and* $\frac{v_\varepsilon}{v_0} < \frac{s}{r}$*, delegation is not optimal* ($x^* = 0$).

Result 4 describes the conditions under which a coalition has nothing to gain from delegating. First, technical uncertainty must intrinsically be a small problem relative to procedural uncertainty ($v_\varepsilon < v_0$). Second, the rate at which agency independence improves technical knowledge must not be too large relative to the rate at which it damages procedural control $\left(\frac{v_\varepsilon}{v_0} < \frac{s}{r}\right)$.

Given $v_\varepsilon < v_0$, $r < s$ is sufficient but not necessary. Result 4 applies when both initial uncertainty about policy consequences and an agency's ability to reduce this uncertainty are relatively low. The political risks of delegation are thus high relative to the informational gains.

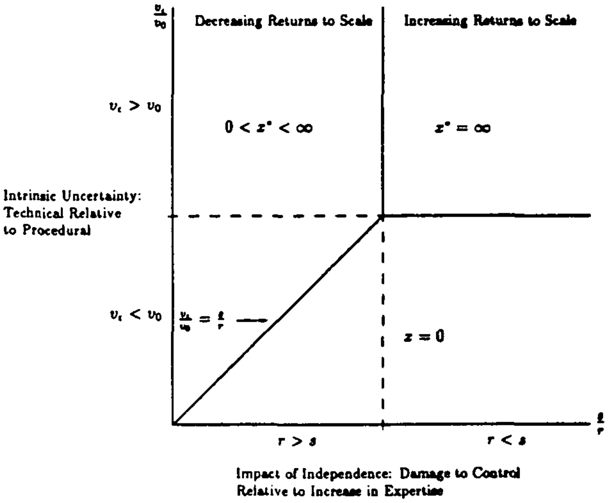
RESULT 5. *When* $v_\varepsilon > v_0$ *and* $r < s$*, the coalition's optimal choice is to give the agency as much independence as possible* ($x^* = \infty$).

In reality, a legislative coalition would never want to give an agency "infinite" independence. At some point, the alternative uses of the resources needed to support high levels of genuine independence, such as relieving technical uncertainty in other policy areas or providing constituency benefits directly, would become more important. The result that "infinite" independence can be optimal occurs because the opportunity costs of resources spent to improve knowledge of policy consequences are omitted from the model. The correct interpretation of result 5 is that it describes conditions under which political control is not a reason for limiting the agency's independence.

Specifically, the coalition will optimally forego all political control in favor of expertise when intrinsic

FIGURE 3

Agency Type as a Function of Policy Characteristics



technical uncertainty is worse than procedural ($v_\varepsilon > v_0$) and when incremental independence harms control faster than it improves expertise ($r < s$). In this case, the coalition faces contradictory forces. Giving the agency more independence damages political control faster than it improves technical knowledge ($r < s$). However, technical uncertainty is an intrinsically bigger problem than procedural uncertainty ($v_\varepsilon > v_0$). The second factor dominates because the negative effect of incremental independence diminishes at the margin.

Figure 3 summarizes results 3–5. The axes show the relative sizes of the exogenous parameters. The ratio v_ε/v_0 is on the vertical axis, s/r is on the horizontal axis. The vertical dashed line shows where $s = r$ and the horizontal dashed line shows where $v_\varepsilon = v_0$. The optimal choice of agency independence is shown in the interior. The three regions denote the three outcomes: a compromise between control and expertise (result 3), no independence and complete control (result 4), or infinite independence and maximum agency expertise (result 5).

The vertical line in the middle of Figure 3 separates the area of decreasing returns to scale in agency independence ($r > s$) from the area of increasing returns ($r < s$). In the area of increasing returns to scale, there is an abrupt transition from optimal procedures that place no constraints on the agency (upper half of the region) to those that constrain the agency completely (lower half). The abrupt shift occurs because of increasing returns to scale. Increasing returns means that if the coalition gains by increasing agency independence from x_1 to x_2, it will gain more by increasing independence further from x_2 to x_3 and more yet from x_3 to x_4. If it pays the coalition to increase independence above zero, it will pay to increase it to infinity. The optimal choice is a corner solution: either give the agency complete independence or none at all. If the agency is given complete independence, technical uncertainty approaches zero and procedural uncertainty is v_0; total uncertainty is v_0. If the agency has no independence, technical uncertainty is v_ε and procedural uncertainty is zero; total uncertainty is v_ε. For the area of increasing returns, if technical uncertainty is the smaller intrinsic problem ($v_\varepsilon < v_0$), the optimal choice is to completely control the agency's induced preferences. If technical uncertainty is the larger problem, the optimal choice is to give the agency complete independence.

An Illustration

Although the four exogenous parameters (r, s, v_ε, v_0) are hard to measure in absolute terms, comparisons about relative parameter values can be made across time, policies, and agencies. Consider, for example, the setting of emission standards for cars and factories. Intrinsic technical uncertainty about pollution from car tailpipes is similar to that about pollution from stationary smokestacks. Intrinsic political uncertainty is different, however, because the interest groups involved are different. The primary opposition to strict emission standards for cars was a single, powerful industry with coherent preferences. In contrast, those hurt by strict stationary source standards are more diverse and more numerous and agree less about what specific standards to oppose. While emission standards affected car manufacturers in basically the same ways, the impact of stationary source standards varied across industries and geographic areas. For example, standards that helped the Western low-sulfur coal industry hurt the Eastern high-sulfur coal industry (Ackerman and Hassler 1981; Crandall 1983). Manufacturing as a sector was less effectively organized for political action than the auto industry.[6]

Intrinsic procedural uncertainty (v_0) was thus higher in the mobile source area, because the desired policy was opposed by a stronger and more unified interest group. Moreover, s was higher in the mobile source area because the behavior of the auto industry was more predictable. In Figure 3, the exogenous parameters for mobile source standards are below and to the right of those for stationary sources. Mobile source standards are thus likely to fall into the region where the optimal choice is to give the agency no independence. This is consistent with the requirements of the 1970 Clean Air Act amendments. No agency expertise was utilized in setting mobile source standards: the numeric limits and their deadlines were set by statute. New source performance standards for stationary sources, in contrast, were to be set by the EPA, using procedures that reflected a compromise between expertise and control.

Comparative Statics and Further Illustrations

Result 3 specifies the cases most relevant to studying legislative delegation. These are the cases where optimal procedures balance expertise and control.

The natural question to ask is how changes in the exogenous parameters affect agency independence.

RESULT 6. *Under the conditions of Result 3, a small increase in* v_ε *will increase the optimal level of agency independence and a small increase in* v_0 *will decrease the optimal level of agency independence, ceteris paribus.*

Result 6 describes the comparative static effects of changes in intrinsic technical and procedural uncertainty. As intrinsic technical uncertainty becomes a bigger problem, the agency is given more independence in order to improve technical knowledge. As the potential for agency drift increases, the coalition gives the agency less independence in order to improve political control.

The comparative static effect of v_ε illuminates differences between agencies created in the early seventies for social regulation. Consider the policy problems of the EPA, Occupational Safety and Health Administration (OSHA) and the Consumer Product Safety Commission (CPSC). The EPA's policies mainly involve health effects; the CPSC's mainly safety; the OSHA is concerned with both. Kelman (1980) discusses two sources of uncertainty in occupational health policy, both of which apply to environmental health as well. First, the sources of health effects are difficult to identify. Did a person contract emphysema from workplace exposure, from breathing polluted air, or from choosing to smoke cigarettes? Second, health effects develop over many years, making causal relationships hard to establish. Neither problem affects safety regulation. Accidents happen at an identifiable point in time, and the source of an accident is typically much clearer than the source of disease.

For both of these reasons, health policies have higher v_ε than safety policies. According to result 6, the greater v_ε, the greater the optimal level of agency independence. The prediction that the EPA's administrative procedures permit greater independence (more expertise and relatively less control) than either the OSHA or the CPSC is consistent with conventional wisdom about these agencies. The EPA has the largest staff of any agency, its own Office of Research and Development, and a number of national laboratories, giving it a great deal of independence (Pederson 1984). The OSHA conducts no research but must depend on the National Institute for Occupational Safety and Health (NIOSH) for technical information (Moe 1989). The CPSC lacks even the independence to set its agenda or propose polices: it must rely on outside groups (typically industry) to research and propose standards (Cornell, Noll, and Weingast 1976).

Although the model was designed primarily to explain decisions about administrative procedures at the national level in the United States, the comparative static results can also be applied across countries. For example, Britain's parliamentary system, combined with disciplined parties, permits fewer ways for interests not represented in the legislative coalition to influence the agency ex post. This means that v_0, intrinsic procedural uncertainty, is lower in the United Kingdom than in the United States. Result 6 predicts that when the British parliament delegates to bureaus, independence should be less restricted by procedures than it is for comparable policy decisions in the United States. The prediction is consistent with Vogel's (1986) empirical study of environmental regulation in the two countries.

The comparative static effects of the rate parameters, r and s, are more complicated. A small increase in the rate at which independence reduces technical uncertainty can sometimes lead to a higher level of agency independence, sometimes to a lower level. The interpretation of this mixed effect is analogous to that of a price change on consumer choices. An increase in r has two effects. First, it changes the rate at which technical uncertainty can be "exchanged" for procedural uncertainty, creating a substitution effect. Second, it expands the choice set of the legislative coalition to include more attractive options. The coalition, if it wanted, could have the same level of technical uncertainty with less procedural uncertainty (or vice versa), creating the equivalent of an income effect. When the optimal level of independence is small, the substitution effect dominates. A small increase in r leads to more agency independence because independence reduces technical uncertainty at a faster rate. When the level of independence is high, the income effect dominates. An increase in r leads to less independence because the same degree of technical uncertainty can be obtained with a lower level of procedural uncertainty.

A parallel argument applies to the effect of s. At low levels of independence, a small decrease in the rate at which independence damages control leads the coalition to allow more independence. When independence is already high, a decrease in s leads to less independence. In this case, the coalition responds to a change that improves its available choices by choosing more control.

CONCLUSION

The most serious limitation of this model is that it treats the legislative coalition as a single decision maker with coherent preferences. When there is general agreement within the coalition about desired outcomes, this simplification is trivial. When there is significant disagreement, however, the choice of administrative procedures will be affected by the institutions that structure legislative bargaining: the committee system, bicameralism, the need for presidential approval. The most promising way to understand how these institutions affect choices of agency procedures is to incorporate them explicitly into models of bargaining over administrative procedures by individuals with different ideal levels of agency independence (Ferejohn and Shipan 1989; McCubbins, Noll, and Weingast 1989). Toward that end, my model can be interpreted as a theory of how individual preferences over administrative procedures derive from preferences over policy outcomes.

Formalizing the deck-stacking argument advances knowledge in three ways. First, it incorporates uncertainty about the effect of administrative procedures as well as about policy outcomes. This accommodates the observation that procedural control works better in some cases than in others. Second, control and expertise do not have to be thought of as all-or-nothing variables. Modeling technical and procedural uncertainty as variances allows us to think of both as matters of degree. Third, results are derived in the context of a standard spatial model with explicit characterizations of preferences and uncertainty.

The advantage of using a spatial model is that I have been able to express precisely the relationship between agency preferences and legislative choices. Agency preferences are influenced—but not completely determined—by legislative decisions. Moreover, the degree of ex ante legislative influence is itself a legislative choice, made through administrative procedures. In choosing administrative procedures, a coalition chooses the distribution from which agency preferences are drawn.

The model shows how a trade-off between control and expertise affects the degree of independence delegated to an agency. A legislative coalition's choice of agency independence, made through the choice of procedures, depends on technical uncertainty about policy consequences, as well as uncertainty about the political environment. The broad prediction of differences in agency procedures across policy areas is consistent with observed heterogeneity of agency independence (Wilson 1980). If legislators care about the consequences of agency decisions, administrative procedures and agency independence will vary according to the technical and political features of the policy area.

APPENDIX

This section proves the results stated in the body of the paper. Tildes (~) denote estimates, stars (*) denote optimal choices or ideal points, and subscripts denote agency (A) or coalition (C).

Result 1. *Optimal administrative procedures give an expected agency ideal point equal to the coalition's ideal point.*

Proof. When delegating, the legislative coalition's problem is

$$\max_{x,\tilde{a}_C} - E_C(z(\tilde{a}_C, \tau(x), \delta(x)) - c^*)^2,$$

where E_C denotes expectation conditional on the coalition's information set. The outcome z is the agency's estimated outcome, $\tilde{z}_A$, plus the random error δ:

$$z = \tilde{z}_A(p^*{}_A(\tilde{a}_C, \tau(x))) + \delta(x). \qquad \textbf{(A-1)}$$

Because the agency makes the policy decision in this case, the agency's information structure is used in the legislative coalition's expectation. The agency's choice of policy, p_A^* is that which maximizes net positive feedback to the agency:

$$\max_p - E_A(z(p, \delta) - a^*)^2.$$

Substituting equation A-1 for z and taking expectations, the agency's problem simplifies to

$$\max_p - (\tilde{z}_A(p)^2 + v_\delta - 2\tilde{z}_A(p) + a^{*2})$$

and the agency's optimal choice of p is that for which

$$\tilde{z}_A(p) = a^*. \qquad \textbf{(A-2)}$$

Substituting the agency's optimal policy (given implicitly in equation A-2) into the coalition's problem gives

$$\max_{\tilde{a}_C,x} - E_C(a^* + \delta - c^*)^2$$

and substituting the coalition's forecast for a^*

$$\max_{\tilde{a}_C x} - E_C(\tilde{a}_C + \tau + \delta - c^*)^2.$$

Taking expectations and making use of the independence of τ and δ reduces the problem to

$$\max_{\tilde{a}_C,x} - (\tilde{a}_{C^2} + v_\tau + v_\delta + c^{*2} - 2\tilde{a}_C c^*).$$

The first-order conditions are $-2\tilde{a}_C + 2c^* = 0$ and

$$-v'_\tau(x) - v'_\delta(x) = 0. \qquad \textbf{(A-3)}$$

Therefore, whenever the second-order conditions are fulfilled (whenever delegation is optimal), the best choice of administrative arrangements sets $\tilde{a}_C = c^*$.

Result 2. *The coalition's expected payoff from delegating to an agency with no independence is the same as from setting the policy itself.*

Proof: From above, the coalition's expected payoff, U_C, from delegating is

$$E_C U_C(\tilde{a}_C,x) = -(\tilde{a}_C^2 + v_\tau + v_\delta + c^{*2} - 2\tilde{a}_C c^*).$$

When $x = 0$, $v_\tau = 0$ and $v_\delta = v_\varepsilon$, so $E_C U_C(c^*,0) = -v_\varepsilon$. If it does not delegate, the coalition maximizes expected payoff according to its own information:

$$\max_p E_C - (\tilde{z}_C(p) - c^*)^2 =$$

$$- (\tilde{z}_C(p)^2 + v_\varepsilon - 2\tilde{z}_C(p)c^* - c^{*2}).$$

The optimal choice is that for which $\tilde{z}_C(p) = c^*$ and the expected payoff associated with this choice is $E_C U_C(c^*) = -v_\varepsilon$.

Result 3. *When* $r > s$ *and* $\frac{v_\varepsilon}{v_0} \geq \frac{s}{r}$, *the optimal level of agency independence is*

$$x^* = \frac{\ln \frac{rv_\varepsilon}{v_0 s}}{r - s}.$$

Proof: The optimal level of independence is given by equation A-3. Substituting for $v_\tau'(x)$ and $v_\delta'(x)$ gives

$$rv_\varepsilon e^{-rx} - sv_0 e^{-sx} = 0. \qquad \textbf{(A-4)}$$

The expression for x^* in equation 1 solves equation A-4. The second-order sufficient conditions,

$$\frac{\partial^2 U_C}{\partial \tilde{a}_C^2} = -2 < 0$$

$$\frac{\partial^2 U_C}{\partial x^2} = v_0 s^2 e^{-sx} - r^2 v_\varepsilon e^{-rx} < 0$$

$$\frac{\partial^2 U_C}{\partial \tilde{a}_C^2}\,\frac{\partial^2 U_C}{\partial x^2} - \left(\frac{\partial^2 U_C}{\partial x \partial \tilde{a}_C}\right)^2 = -2(v_0 s^2 e^{-sx} - r^2 v_\varepsilon e^{-rx}) > 0,$$

are satisfied at x^* whenever $r > s$.

Result 4. *When* $v_\varepsilon < v_0$ *and* $\frac{v_\varepsilon}{v_0} < \frac{s}{r}$, *delegation is not optimal* ($x^* = 0$).

Proof: There are two cases.

Case a. $v_\varepsilon < v_0$, $\frac{v_\varepsilon}{v_0} < \frac{s}{r}$ *and* $r > s$. Because $r > s$, the second-order conditions for a maximum are satisfied at the value of x^* in equation 1. However, this is a negative number because $\frac{v_\varepsilon}{v_0} < \frac{s}{r}$. For all positive values of x, $\frac{\partial EU_C}{\partial x} < 0$. The coalition's best choice is $x = 0$.

Case b. $v_\varepsilon < v_0$, $\frac{v_\varepsilon}{v_0} < \frac{s}{r}$ and $r < s$. For this case, there is a positive value of x for which the first-order conditions are fulfilled, but the second-order sufficient conditions for a *minimum* hold. Therefore, the maximum expected political benefits occur at one of the limiting values of x; either $x^* = 0$ or $x^* = \infty$. Since

$$E_C U_C(c,0) = -v_\varepsilon \text{ and } \lim_{x \to \infty} E_C U_C(c,x) = -v_0,$$

the condition that $v_0 > v_\varepsilon$ means that $x^* = 0$.

Result 5. *When* $v_\varepsilon > v_0$ *and* $r < s$, *the coalition's optimal choice is to give the agency as much independence as possible* ($x^* = \infty$).

Proof: As in case b of result 4, $r < s$ implies that the only critical point is a minimum. The optimal level of independences is either zero or infinity. Now, because $v_\varepsilon > v_0$, $x^* = \infty$ gives the highest political benefits.

Result 6. *Under the conditions of Result 3, a small increase in* v_ε *will increase the optimal level of agency independence and a small increase in* v_0 *will decrease the optimal level of agency independence, ceteris paribus.*

Proof: Differentiating equation 4 with respect to v_ε and v_0 gives

$$\frac{\partial x^*}{\partial v_\varepsilon} = \frac{1}{v_\varepsilon(r-s)} > 0 \text{ whenever } r > s$$

$$\frac{\partial x^*}{\partial v_0} = \frac{-1}{v_0(r-s)} < 0 \text{ whenever } r > s.$$

To see why there is no clear comparative static result for r, observe that

$$\frac{\partial x^*}{\partial r} = \frac{1}{r-s}\left(\frac{1}{r} - \frac{\ln \frac{rv_\varepsilon}{sv_0}}{r-s}\right)$$

has both positive and negative values. Because the second term in the parentheses is x^*, however, one can infer that an infinitesimal increase in r will lead to an increase in independence when independence is low and a decrease when it is high.

Similarly,

$$\frac{\partial x^*}{\partial s} = \frac{1}{r-s}\left(\frac{-1}{s} + \frac{\ln \frac{rv_\varepsilon}{sv_0}}{r-s}\right),$$

which is positive when x^* is high and negative when it is low.

Notes

I would like to thank Linda Cohen, Jim DeNardo, Art Denzau, David Epstein, Tom Gilligan, Susanne Lohmann, Roger Noll, Sharyn O'Halloran, Mary Olson, Tom Romer, Barry Weingast, Robyn Wornall and seminar participants at Stanford, Washington University, University of Southern California and the University of Illinois for their comments and suggestions. The support of the University of California at Los Angeles' Center for American Politics and Public Policy is gratefully acknowledged.

1. Other important articles in this vein include McCubbins and Schwartz's (1984) model of "fire alarm" oversight, McCubbins's (1985) analysis of agency discretion as a legislative choice, and Moe's (1989) analysis of the effects of legislative bargaining on decision making in the EPA, OSHA, and CPSC.

2. While it is often casually assumed that the Supreme Court's 1983 decision in *Chadha* v. *Immigration and Naturalization Service* made void all legislative veto provisions, this is not the case. Many types of legislative veto remain constitutional. See Martin (1992) for a taxonomy of legislative vetoes and an analysis of how this particular type of administrative procedure affects policy outcomes.

3. See Bendor 1989 for a review of older models of bureaucratic decision making, many of which assume that agency preferences are defined over budget or effort, rather than policy outcomes. An advantage of the external signals model is that it links these realistic-sounding preference specifications to a usable assumption about how agencies will make policy choices.

4. Note that this model does not address the issue of legislative coalition's information ex post. This will vary depending on the particular procedures used to generate the desired level of independence. The signaling game approach is better suited to answer this question.

5. The model omits the budget costs of agency research from the legislative coalition's decision. In reality, creating an

agency with substantive independence requires resources, and those resources have opportunity costs. The model thus assumes that the opportunity cost of agency resources is not a decisive factor in the choice of administrative procedures. This amounts to assuming that an incrementally higher agency budget has less impact on legislators than costs imposed on constituents by bad policy decisions.

6. The seminal work on factors that predispose groups to organize effectively for political action is Olson 1965. See Noll and Owen 1983 on how the organization of interests has affected U.S. regulatory policy.

References

Aberbach, Joel. 1990. *Keeping a Watchful Eye*. Washington: Brookings Institution.

Ackerman, Bruce, and William Hassler. 1981. *Clean Coal/Dirty Air*. New Haven: Yale University Press.

Arnold, R. Douglas. 1987. "Political Control of Administrative Officials." *Journal of Law, Economics, and Organizations* 3:279–86.

Baron, David, and Roger Myerson. 1982. "Regulating a Monopolist with Unknown Costs." *Econometrica* 50:911–30.

Bawn, Kathleen. 1992. "Institutional Arrangements and Legislative Behavior: The Politics of Institutional Choice." Ph.D. diss., Stanford University.

Bendor, Jonathan. 1989. "Formal Models of Bureaucracy." *British Journal of Political Science* 18:353–95.

Calvert, Randall, Mathew McCubbins, and Barry Weingast. 1989. "A Theory of Political Control and Agency Discretion." *American Journal of Political Science* 33:588–611.

Cornell, Nina, Roger Noll, and Barry Weingast. 1976. "Safety Regulation." In *Setting Domestic Priorities*, ed. H. Owen and C. Schultze. Washington: Brookings Institute.

Crandall, Robert. 1983. *Controlling Industrial Pollution*. Washington: Brookings Institute.

Epstein, David, and Sharyn O'Halloran. 1993. "Administrative Procedures, Information, and Agency Discretion: Slack Versus Flexibility." Columbia University. Typescript.

Ferejohn, John, and Charles Shipan. 1989. "Congressional Influence on Administrative Agencies: A Case Study of Telecommunications Policy." In *Congress Reconsidered*, 4th ed., ed. Lawrence Dodd and Bruce Oppenheimer. Washington: Congressional Quarterly.

Fiorina, Morris. 1982. "Legislative Choice of Regulatory Forms." *Public Choice* 39:33–61.

Gilligan, Thomas, and Keith Krehbiel. 1987. "Collective Decisionmaking and Standing Committees: An Informational Rationale for Restrictive Amendment Procedures." *Journal of Law, Economics, and Organizations* 3:287–335.

Gilligan, Thomas, William Marshall, and Barry Weingast. 1989. "Regulation and the Theory of Legislative Choice: The Interstate Commerce Act of 1887." *Journal of Law and Economics* 32:35–61.

Hamilton, James, and Christopher Schroeder. 1994. "Strategic Regulators and the Choice of Rulemaking Procedures: The Selection of Formal Versus Informal Rules in Regulating Hazardous Waste." *Law and Contemporary Problems* 57:111–60.

Hill, Jeffrey, and James Brazier. 1991. "Constraining Administrative Decisions: A Critical Examination of the Structure and Process Hypothesis." *Journal of Law, Economics, and Organizations* 7:373–400.

Horn, Murray, and Kenneth Shepsle. 1989. "Commentary on Administrative Arrangements and the Political Control of Agencies: Administrative Process and Organizational Form as Legislative Responses to Agency Costs." *Virginia Law Review* 75:499–505.

Kelman, Steven. 1980. "Occupational Health and Safety Administration." In *The Politics of Regulation*, ed. James Q. Wilson. New York: Basic Books.

Kiewiet, D. Roderick, and Mathew McCubbins. 1991. *The Logic of Delegation*. Chicago: University of Chicago Press.

Lupia, Arthur, and Mathew McCubbins. 1994. "Designing Bureaucratic Accountability." *Law and Contemporary Problems* 57:91–126.

Magat, Wesley, Alan Krupnick, and Winston Harrington. 1986. *Rules in the Making: A Statistical Analysis of Regulatory Agency Behavior*. Washington: Resources for the Future.

Martin, Elizabeth. 1992. "Fast Track and Free Trade: Why Does Congress Delegate?" Presented at the annual meeting of the Midwest Political Science Association, Chicago.

McCubbins, Mathew. 1985. "The Legislative Design of Regulatory Procedure." *American Journal of Political Science* 29: 721–48.

McCubbins, Mathew, Roger Noll, and Barry Weingast. 1987. "Administrative Procedures as Instruments of Political Control." *Journal of Law, Economics, and Organization* 3:243–77.

McCubbins, Mathew, Roger Noll, and Barry Weingast. 1989. "Structure and Process; Politics and Policy: Administrative Arrangements and the Political Control of Agencies." *Virginia Law Review* 75:431–82.

McCubbins, Mathew, Roger Noll, and Barry Weingast. 1990a. "Public Interest and Structure-induced Policy." *Journal of Law, Economics, and Organizations* 6:203–12.

McCubbins, Mathew, Roger Noll, and Barry Weingast. 1990b. "Positive and Normative Models of Procedural Rights: An Integrative Approach to Administrative Procedures." *Journal of Law, Economics, and Organizations* 6:307–32.

McCubbins, Mathew, and Thomas Schwartz. 1984. "Congressional Oversight Overlooked: Police Patrols Versus Fire Alarms." *American Journal of Political Science* 28:167–79.

Moe, Terry. 1989. "The Politics of Bureaucratic Structure." In *Can the Government Govern?*, ed. John Chubb and Paul Peterson. Washington: Brookings Institution.

Moe, Terry. 1990. "Political Institutions: The Neglected Side of the Story." *Journal of Law, Economics, and Organizations* 6:213–53.

Noll, Roger. 1971. *Reforming Regulation*. Washington: Brookings Institution.

Noll, Roger. 1983. "The Political Foundations of Regulatory Policy." *Zeitschrift für die gesamte Staatswissenschaft* 139:377–404.

Noll, Roger. 1985. "Government Regulatory Behavior: A Multidisciplinary Survey and Synthesis." In *Regulatory Policy and the Social Sciences*, ed. Roger Noll. Berkeley: University of California Press.

Noll, Roger, and Bruce Owen. 1983. *The Political Economy of Deregulation*. Washington: American Enterprise Institute.

Olson, Mancur. 1965. *The Logic of Collective Action*. Cambridge: Harvard University Press.

Olson, Mary. 1994. "Regulatory Agency Discretion among Competing Industries: Inside the FDA." Washington University, St. Louis. Typescript.

Pederson, William. 1984. "Environmental Protection Agency." In *Federal Regulatory Processes: Agency Practices and Procedures*, ed. Gary and Jerome Nelson. Washington: Harcourt, Brace, Jovanovich.

Robinson, Glen. 1989. "Commentary on Administrative Arrangements and the Political Control of Agencies: Political Uses of Structure and Process." *Virginia Law Review* 75:483–98.

Vogel, David. 1986. *National Styles of Regulation*. Ithaca: Cornell University Press.

Weingast, Barry, and Mark Moran. 1983. "Bureaucratic Discretion or Congressional Control? Regulatory Policy and by the Federal Trade Commission." *Journal of Political Economy* 91:765–800.

Wilson, James Q. 1980. *The Politics of Regulation*. New York: Basic Books.

Kathleen Bawn is Assistant Professor of Political Science, University of California at Los Angeles, Los Angeles, CA 90024-1472.

CHOOSING THE BEST SOCIAL ORDER: NEW PRINCIPLES OF JUSTICE AND NORMATIVE DIMENSIONS OF CHOICE

GRZEGORZ LISSOWSKI *University of Warsaw*
PIOTR SWISTAK *University of Maryland*

There are many standard rules of aggregating individual preferences; majority rule is but one example. These rules determine what we typically consider to be a fair or a just solution to the problem of social choice. Yet we know very little, either about how these solutions relate to what a person, not a rule, would choose as a fair outcome or about what ethical or political beliefs would guide people in their choices. An empirical study was conducted to address these problems. As standard normative solutions fail to explain choices obtained in the study, we propose a new set of solutions, which generalize two classical principles of justice: Rawlsian and conservative. These generalized solutions fit the data remarkably well. More important, they uncover two normative dimensions within which choices turn out to be very consistent. These dimensions, we conjecture, indicate subjects' underlying attitudes. We use this theory to compare ethical-political attitudes in samples from Poland, Japan, and the United States.

With the emergence of new democracies in Eastern Europe, theorists of social choice joined briefly with the pragmatic legacy of Marquis de Condorcet as they went on to advise the newly elected legislatures about the merits and the perils of the various ways they may choose to constitute themselves and their nations. This has provided a good opportunity to review the field's abilities and limitations, always a useful exercise. We have been reminded, for instance, that while we know plenty about formal properties of the many voting methods that we use, we know practically nothing about which properties people might find more desirable and why. Neither can we say much about people's judgment of outcomes. Yet since in politics we usually use outcomes to argue for or against certain aggregating procedures or other political institutions, it would clearly be important to know which solutions people judge as fair and what values and beliefs affect their judgments.

In a sense, it is also the main deductive result of the social choice theory that leads, seemingly, to the same set of concerns. The basic problem of the social choice theory is how to aggregate individual preferences into the single best outcome for the society. The famous answer, known as Arrow's Theorem, implies that there is no good way to do it: any method of aggregation has to violate some conditions of fairness, hence all methods of voting are intrinsically flawed.[1] Thus none of the voting methods can be considered as an ethically neutral or a value-free solution. Wouldn't this suggest, however, some relationship between the values that groups or societies adhere to and the rules of aggregation they use? If so, to what extent may the choice of a voting rule be affected by ideologies, values, beliefs, or ethical attitudes? What would such beliefs be, and how would they affect judgments of what is a fair social choice outcome?

Yet all this may sound far fetched: Arrow's Theorem is based, after all, on normative, not positive, premisses; and its proof has a relatively complex structure. These are more than sufficient reasons to make one skeptical about the theorem's empirical implications. Thus the whole issue requires caution: there are ways in which social choice can be linked with empirical studies or observations, but there are ways in which it cannot.[2]

The beginnings of what we now call social choice theory have actually been motivated by very practical concerns. Initially, the problem was how to improve the existing rules of group decision making (see Black 1958, pt. 2). The adequacy of the postulated properties of such rules has been discussed extensively, though on the purely theoretical grounds of ethics. No empirical questions about which properties may be more acceptable than others in various conflict situations have ever entered this discussion. Some 40 years later, such empirical research is still scarce. Studies comparing results of theoretically derived rules with procedures used in practice have been rare (see Fishburn 1986); so have been studies comparing acceptability of various normative principles. Notable experimental work has been done in a related area of distributive justice. Some researchers have analyzed peoples' evaluations of various principles of justice (see Frohlich and Oppenheimer 1992; Frohlich, Oppenheimer, and Eavy 1987; Lissowski, Tyszka, and Okrasa 1991). Others have studied empirical distributions of goods obtained in experiments comparing them with distributions normatively prescribed by different principles of justice (see Lissowski 1993a, 1993b; Yaari and Bar-Hillel 1982). Yet with the single exception of the work by Tomiyama and Sayeki (1982), we are not aware of any similar research related to social choice proper. Naturally, there are reasons for the lack of empirical work, and some of them are no doubt related to the fact that the positive aspects of social choice have never been clear. A brief look at the anatomy of the problem will help to clarify its nature.

The problem of social choice can be seen as consisting of three analytical components: the rule of aggregation, a set of properties of the rule, and the set of outcomes that the rule generates in specific situations. Any of the three elements can be an independent object of empirical research: we can investigate peoples' preferences over rules, rules' properties, and rules' outcomes. An obvious concern, however, would be if the theoretical relations that hold among these elements would be preserved by peoples' choices. Neither full consistency nor complete independence are very likely.

Some directions of research seem, however, to be more promising than others. We know, for instance, that people may approve of certain principles or their normative properties and yet do not approve of the outcomes that have to follow (see Ebbesen and Konecni 1980; Lissowski 1993a). Since it is the outcomes that ultimately matter, they, more than anything else, should reflect the true beliefs and values that rule peoples' actions. Outcomes should thus provide a more reliable index of the actual beliefs than what people may be able to (or may want to) admit.[3] Is it possible to use outcomes to infer attitudes, and would these attitudes indeed prove to persist across various contexts? These problems are objects of an empirical study and a series of theoretical results that we present in this paper.

The empirical study was conducted in Poland in 1988. We describe it and analyze how well the observed outcomes can be explained by the standard social welfare functions. The fit turns out to be very poor, and the data indicate that we should approach the problem as one of distributive justice. We show that there is one natural way to represent a social choice problem as a problem of distributive justice. This is because the distance function, necessary to establish this correspondence, is uniquely determined by a set of normative conditions that ought to be met by a fair social choice procedure. With this natural distributive justice representation, we can next ask how the outcomes of the standard principles of justice relate to the observed choices. Again, we find that they fail to explain the data. Yet we also find that the structure of distances is very closely related to the observed distributions. This leads us to a new class of principles of justice, which generalize two standard principles: Rawlsian and conservative. When applied to the data, the generalized principles provide an almost perfect fit showing, in addition, a very high consistency of choices within two normative categories: Rawlsian and conservative. People, in other words, consistently choose within the generalized Rawlsian and conservative categories. Finally, we turn to two other data sets, obtained independently in Japan and the United States. Using our theory, we compare political attitudes in samples from the three countries.

THE STUDY

The study was conducted in Warsaw in 1988 and involved 144 undergraduate students; it concluded with 140 valid responses. In the first part of the study, subjects were asked to choose the best social order in eight situations of conflicting individual preferences. The situations were described as a jury problem where three jurors state their decisions on how to award three prizes to three candidates. In each of the eight cases, subjects were given jurors' rankings of three candidates (individual preferences) and were asked to choose the best verdict of the jury (social order). A list of all 13 theoretically possible verdicts (see Table 2 and the Appendix) was included in the instruction sheet, as was a schematic representation of the eight situations. Candidates were identified by letters (A, B, C), jurors by their preference orders. A situation was, then, no more than a set of individual preferences. To keep with convention, we call a set of individual preferences a *profile*. For instance, ABC, BCA, CBA would have been used to denote a profile where the first juror ranks A over B over C, the second B over C over A, and the third C over B over A. We use strings of alternatives, like ABC, to denote a strict order. If an order is not strict and two alternatives, say A and B, are tied, we write A–B to indicate a transitive indifference relation between them. As a result A–BC, for example, means A is judged to be as good as B, and they are both judged to be better than C.

The issue of the number and the form of the profiles used in the study requires a comment. We should first note that the set of all 3 × 3 profiles can be partitioned into eight nontrivial classes[4] in such a way that elements of the same class will differ only with respect to the order in which jurors' preferences are listed and/or the way alternatives are labeled. The three profiles ABC, ABC, CBA; CBA, ABC, ABC; and CBA, CBA, ABC—for example—belong to one class, since 1 and 2 differ with respect to order, 1 and 3 with respect to labeling, and 2 and 3 with respect to both order and labeling. As we look at profiles belonging to the same class, they strike as different symbolic representations of the same social choice problem. We would expect, in consequence, that no matter what one thinks a solution should be, it should be essentially the same (i.e., the same after relabeling of the alternatives) for all profiles in the class. Clearly, it is the intrinsic aspects of the problem, not the form in which the problem is represented, that ought to decide what a just solution would be. This is a normative statement however, which is equivalent to saying that neither the labeling of alternatives nor the labeling (here: order) of jurors should matter. These are, in fact, the standard neutrality and anonymity conditions that are required of fair aggregation procedures.

Anonymity and neutrality should, in fact, follow from the design. The abstract form in which alternatives and jurors are represented ought to imply an impartial treatment of both. Yet the problem is empirical, and it remains to be seen whether, indeed, subjects' choices are unaffected by the order of jurors' preferences and the labeling of alternatives. The study was designed to test this (see the Appendix). With the design controlling for the effects of order and labeling, as well as their interaction, the data proved none of the three factors to be significant at the .05 level. This finding is important for a number of reasons. First, anonymity and neutrality make the data compatible with all standard rules of choice. Second, the absence of significant effects can be treated as an index of the "quality" of data; it implies a small incidence of random or erratic choices, and this will continue to be important as we proceed to analyze the data with increasingly more complex models. Finally, the third advantage of neutrality and anonymity is of a more practical nature: since the specific form of the profile representation does not matter, we will be able to use, in all tables and figures to follow, a single version of the many formally equivalent profiles that were actually employed in the study.

The choice of the 3 × 3 profiles was motivated by the following considerations. First, a 3 × 3 profile is easy to comprehend. Second, with only 13 social orders possible, the task of selecting the single best one is simple. And third, with only eight distinct classes of profiles, subjects can be presented with the full set of eight in a short and undemanding study. All this accounts for a cognitive simplicity that we believe has maximally reduced choices made on other than purely normative grounds. We will present the full set of data from this study later. First, however, we look at standard normative solutions and examine how they relate to the observed choices. We begin this analysis with the standard social welfare functions.

EMPIRICAL CHOICES AND SOCIAL WELFARE FUNCTIONS

The concept of the social welfare function denotes *any* rule that for any profile of individual preferences prescribes exactly one social order.[5] This general notion does not contain normative qualifications. Yet since we plan to compare the outcomes judged as fair with the outcomes of social welfare functions, it would only make sense to consider functions that are regarded as fair or democratic (we will call them here *standard*).[6] The objective of this section is to examine how the outcomes of the standard social welfare functions compare with the empirical choices obtained in the study.

Before we turn to the data, a brief note on the set of the standard welfare functions. It turns out that with the set of profiles used in the study, we can only separate three classes of functions that prescribe different outcomes. More specifically, the three classes are such that functions from the same class choose identical outcomes in all eight profiles, and functions from different classes choose outcomes that differ in at least one profile. The three classes are majority-related rules, positional rules, and Borda elimination rule.[7]

When we think about a majority rule, for instance, what we have in mind is a specific mechanism of aggregation. (Deciding between two alternatives A and B, majority rule compares the number of those who prefer A over B to the number of those who prefer B over A. Depending on how the two numbers compare, the rule declares a winner or a tie.) We rarely, if ever, think about a rule in terms of its normative properties. Yet any rule (like any object) can be thought of as equivalent to a specific set of properties.[8] This analytical correspondence between rules and their properties will be helpful in looking for the normative basis of peoples' choices.

To see if peoples' choices are consistent, across all profiles, with normative conditions corresponding to one of the three groups of social welfare functions, we need to compare the three sequences of choices prescribed by these functions in eight profiles with sequences of eight choices obtained in the study. For convenience, we will use the name *empirical distribution* to refer to a vector of eight social choices made by a subject. Thus we can say that our data consists of 140 empirical distributions. We start our analysis by noting some interesting properties of this set.

Empirical distributions turned out to be very diverse. In the set of 140 there are 96 different distributions and 88 of them are unique. This means that 88 subjects, or 62.9% of the group, made a series of choices that were not made by anyone else. Only 8 of the total of 96 distributions were chosen by more than one person; the number of subjects who chose these eight distributions ranged from the minimal of 2 to the maximal of 16.

Table 1 presents a truncated set of empirical distributions and compares them with outcomes of standard social welfare functions. The most frequent distribution (No. 1 in Table 1), used by 11.4% of subjects, corresponds to social choices under the positional rules; the fourth most frequent (4.3%) to choices under the majority rule; and the fifth most frequent (2.9%) to choices under the Borda elimination rule. Standard social welfare functions agree with 18.6% of the empirical distributions. This, needless to say, leaves the bulk of the data unexplained.

Yet such analysis places some very stringent constraints on subjects' choices: standard social welfare functions are very demanding in all the normative conditions they satisfy and it would not be realistic to expect actual choices to follow such a strenuous standard. Note, for instance, that although the second (9.3%) and the third (5.0%) most frequent distributions do not fit the sequences of choices obtained with the standard welfare functions, the differences are very small: distribution No. 2 differs from the majority rule distribution (No. 4) in one place (ACB in profile II), and distribution No. 3, in two places (CBA in profile I and ACB in II).[9] This observation raises the question of how well social welfare functions can explain choices within each separate profile. Such

TABLE 1

Empirical Distributions from the Polish Study Compared with Standard Social Welfare Functions

PROFILE		EMPIRICAL DISTRIBUTIONS IN ORDER OF HOW FREQUENTLY THEY WERE CHOSEN										
		NO. 1[a]	NO. 2	NO. 3	NO. 4	NO. 5	NO. 6	NO. 7	NO. 8	NO. 9	. . . .	NO. 96
I	ABC BCA CBA	**BCA**	BCA	CBA	**BCA**	**BCA**	BCA	BCA	CBA	BCA	. . . .	BA–C
II	ABC BCA ACB	**ABC**	ACB	ACB	**ABC**	**ABC**	AB–C	ABC	ACB	BAC	. . . .	AB–C
III	ABC BCA BAC	**BAC**	BAC	BAC	**BAC**	**BAC**	BAC	BAC	BA–C	BAC	. . . .	BAC
IV	ABC ABC BCA	**A–BC**	ABC	ABC	**ABC**	**ABC**	ABC	A–BC	ABC	ABC	. . . .	A–BC
V	ABC ABC CAB	**AB–C**	ABC	ABC	**ABC**	**AB–C**	AB–C	ABC	ABC	ABC	. . . .	AB–C
VI	ABC ABC CBA	**ABC**	ABC	ABC	**ABC**	**ABC**	ABC	ABC	ABC	ABC	. . . .	AB–C
VII	ABC ABC ACB	**ABC**	ABC	ABC	**ABC**	**ABC**	AB–C	ABC	ABC	ABC	. . . .	ABC
VIII	ABC ABC BAC	**ABC**	ABC	ABC	**ABC**	**ABC**	ABC	ABC	ABC	ABC	. . . .	ABC
Percentage of Subjects Choosing[b]		**11.4 (16)**	9.3 (13)	5.0 (7)	**4.3 (6)**	**2.9 (4)**	1.4 (2)	1.4 (2)	1.4 (2)	.7 (1)	. . . .	.7 (1)

Note: Total number of valid cases N = 140.

[a] Columns with bold entries correspond to empirical distributions that are explained by the standard social welfare functions: no. 1 by positional rules, no. 4 by majority-related rules, and no. 5 by Borda elimination rule.

[b] Number of cases are in parentheses.

within-profile analysis does not entail the constraining assumption that all choices in an empirical distribution are consistent with a single function from our set: we allow here for a possibility that subject's choices follow an unspecified set of conditions that generate outcomes that agree, in each profile, with a choice prescribed by one of the standard functions (though not the same one in all profiles).

To better understand the meaning of such analysis, consider the following abstract example. Suppose there are only two standard rules with properties {P, Q} and {Q, T}. Assume, moreover, that none of the observed empirical distributions agrees with a single rule across all profiles, yet they all agree with one rule in some profiles and the second rule, in others. This scenario yields 0% agreement with empirical distributions and 100% agreement with choices in the within profiles analysis. For the purpose of making inferences about normative dimensions of choices, both the across-profiles and the within-profiles analyses yield important information: the first tells us that neither the {P, Q} nor the {Q, T} are consistently preserved by the actual choices, and the second indicates that Q may be the common normative dimension of choice. It is precisely this second contingency that we assume possible as we undertake the within-profiles analysis. Yet a brief look a the data indicates, again, an obvious lack of fit.

The percentages of choices that cannot be explained by the standard social welfare functions are 44.2%, 66.4%, 22.6%, 7.8%, 12.2%, 21.5%, 18.5%, and 21.4% in profiles I through VIII, respectively. The fit is far from satisfactory. The rather poignant prediction gaps in profile II and I exclude the possibility of using this framework to account for the actual choices.

To summarize our findings, standard social welfare functions fail to explain 81.4% of empirical distributions in the across-profiles analysis and up to 66.4% of choices within profiles. While the failure to predict need not be surprising, the degree of incongruity is rather conspicuous. One reason that may account for the size of this gap will lead us, in the next section, to a new conceptual framework.

DISTRIBUTIVE JUSTICE REPRESENTATION OF THE SOCIAL CHOICE PROBLEM

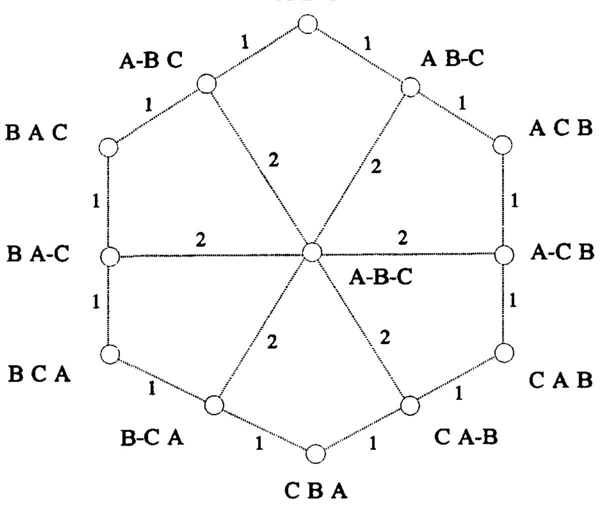
Normative properties of standard rules of aggregation are motivated by a just treatment of voters. The common understanding of justice is sufficiently broad however, to entail outcomes that need not be consistent with those implied by the standard rules of aggregation. Tomiyama and Sayeki point out, for instance, that "equity in the treatment of voters . . . does not guarantee *equity in reflection of voters' preference orderings*" (1982, 98). To illustrate this point, they propose to consider the ABC, ABC, CBA profile (profile VI in our study). For this profile, all standard social welfare functions prescribe ABC (see Table 1). Yet one may object to ABC's being just on the grounds that it "perfectly reflects" the first two preference orders and "completely ignores" the third one. If this aspect of justice matters in peoples' judgments, ABC would not be their choice of a social order. Indeed, in Tomiyama and Sayeki's study only 39% of 70 subjects prescribed ABC as the best outcome for this profile, while the remaining 61% picked orders (mainly BAC and ACB) that were "closer to" the preference order of the third voter.

To account for the fact that one order "perfectly reflects," or "completely ignores," or "is closer to" another one, we need some measure of distance on preferences. Such measure was proposed by Kemeny (1959) and Kemeny and Snell (1962). They formulated a set of conditions that one would naturally expect to be satisfied by such measure and proved that there is exactly one measure that meets them all. Once we have a distance on preferences, the geometric representation of the social choice problem is very natural: as we conceive of individual preferences as points in a space, we would naturally think about selecting a social order as a point that is close to them in some sense. For a point like this to be acceptable as a fair social choice, however, the measure itself has to satisfy basic normative conditions that we would usually impose on a fair social choice procedure. If it does not, using it cannot, in general, yield fair solutions. This is a critical point in the construction, yet one that has been missed in Kemeny's and all subsequent accounts. We resolve this problem in the Appendix. There we first construct a set of normative conditions that are typically required of any fair social choice procedure. Next we show that the Kemeny–Snell measure is, in fact, the only measure that satisfies them all.

Fortunately, we will not need to get into any more technical details of the Kemeny–Snell measure. What is sufficient for our presentation is the case of three alternatives and this can be easily pictured by a distance map of Figure 1. The shortest distance in this map is taken as a unit and the distance between any two orderings is defined as the length of the shortest path (along the dotted lines) that connect them. For example, the distance between A–BC and ABC is 1, the distance between A–BC and BCA is 3 (1 + 1 + 1), and the distance between A–BC and CBA is 5 (1 + 1 + 1 + 1 + 1 or 2 + 2 + 1). In this last case we can see that there may be more than one shortest path connecting two orderings.

FIGURE 1

Distances between All Rankings of Three Alternatives



Distances between individual preferences and potential social orders are necessary if we want to use principles of justice to select the best social choice outcomes. This is because principles of distributive justice, as opposed to social welfare functions, require that we have individuals' utilities of each potential social choice outcome. Certainly, principles of distributive justice were developed in a very different context—to deal with the problem of the division of a good. In the standard context of distributive justice, the set of outcomes consists of all possible divisions of a good, and the amounts ascribed to various individuals need not be equal. When choosing the best social order, it is meaningless to talk about the amount of a good that is to be distributed. Yet it makes sense to talk about the level of satisfaction of each person from the adopted order. Together with assumptions about individuals' utilities—their form and comparability—this would indeed be sufficient to frame the problem of social choice as one of distributive justice.

Hence, to have our problem fully defined as one of distributive justice, we need to specify assumptions about individual utility functions and about their interpersonal comparability. This is what we do in the following three assumptions.

Assumption 1 (Utility of Distances). *For any individual* $j \in J$ *(here: juror), there is a real-valued utility function* u_j *defined on the set of distances between* j*'s preference and social choice orders* $R \in \mathfrak{R}$ *such that*

A (*Cardinal Utility of Distances*). u_j *is a strictly decreasing linear function.*

B (*Ordinal Utility of Distances*). u_j *is an arbitrary strictly decreasing function.*

The two versions of this assumption specify different levels of utility measurement: 1A implies that utilities are measurable on an interval scale (i.e., there is some arbitrary unit of measurement that makes differences between utilities meaningful); and 1B, which is a weaker condition, implies that utilities are measurable on an ordinal scale (i.e., all we can say is that one utility is larger than another, but we can not say by how much). In both versions, utilities are assumed to be a function of the Kemeny–Snell measure between orders (individual and social) and are assumed to be inversely related to these distances: the larger the distance, the smaller its utility.

ASSUMPTION 2 (Identity of Individual Utility Functions). *Utility functions of all individuals (here: jurors) are identical.*

As descriptive of conditions met in the empirical study this is a particularly natural and obvious assumption: jurors, after all, are theoretical objects and the design of the study does not provide any information that would justify treating some of them differently than others. The relevance of our next assumption is no less obvious.

ASSUMPTION 3. (Interpersonal Comparability of Utilities). *Utilities of all individuals (jurors, in our study) are fully comparable.*

It is important to remember that all principles of distributive justice require some assumptions about the level of measurement of utilities and their interpersonal comparability; and principles differ with respect to the assumptions on which they are based. Utilitarian principle, for instance, assumes cardinal utilities and a common unit of measurement, while Rawls' lexicographic maximin assumes ordinal utilities and full comparability of utility levels among all individuals (see Kern 1978; Lissowski 1986; Sen 1974).

It is often required that principles of justice meet a condition of impartiality. Suzumura (1983) proposed to consider as impartial only those principles of justice which include the Suppes' justice relation. This relation is defined as follows

SUPPES' GRADING PRINCIPLE OF JUSTICE. *Let* J *be a set of individuals and* T_J *a set of all permutations of* J *(i.e., one-to-one functions from* J *to* J*). Social choice* R *is more just than* R* *if there is a permutation* $t \in T_J$ *such that the utility of* R *for every individual* $j \in J$ *is greater than or equal to the utility of* R* *for* t(j) *and there is an individual* $i \in J$ *for whom this utility is strictly greater.*[10]

The idea of the grading principle is to compare a vector of utilities of social choice *R* with that of social choice *R** by permuting one of them and then comparing the two, using the criterion of strong Pareto optimality. Impartiality condition is implicit here in that we allow comparisons between permuted utilities. Information about which utility is whose, (i.e., an individual-specific information) is assumed to have no ethical value and hence no influence on the choice of social order—this is the core idea of impartiality.

Impartiality, we should note, differs from the anonymity condition, which requires that social choice remains the same under all assignments of utility functions (or preferences) to individuals. It also differs from the neutrality condition, which requires invariance of social choice under any relabeling of alternatives. Yet while different from the two, impartiality combines some basic elements of both, in that it is a very fundamental condition. As one would expect, impartiality is satisfied by all standard principles of justice. All these principles evaluate a vector of individual utilities with respect to one or more indices; values of these indices are used to order all vectors of utilities from the most just to the least just.

EMPIRICAL CHOICES AND THE STANDARD PRINCIPLES OF JUSTICE

With assumptions 1–3 and the impartiality condition the task of representing social choice problem is considerably easier. First, instead of individual utilities we can consider distances between jurors' preferences and possible social orders[11] (keeping in mind, however, that utilities are strictly decreasing functions of distances). This is possible because of assumptions 1–3. Second, instead of the original vectors of distances, we can consider vectors with coordinates ordered from the smallest to the largest; this is possible because of the impartiality condition. We will denote the set of ordered vectors as Δ. For all $D \in \Delta$, where $D = (d_1, \ldots, d_n)$, we thus have $d_i \leq d_{i+1}$ for all $i = 1, \ldots, n - 1$.[12] In Table 2, distances between individual preferences of the three jurors in profile I and the 13 potential social choice orders have been presented in two different forms, one using the original, the other, the ordered vectors. Profile I, as analyzed in Table 2, will serve as a running example; for now, we hope that it clearly shows the benefits of carrying out the analysis on the set of ordered, as opposed to the original, vectors.

Principles of justice are typically defined through the specification of a "more just than" relation on a set of social alternatives. The definition of the Suppes' grading principle illustrates this point. All that we usually need however, is not the entire order structure on the domain of social alternatives but only the maximal elements in it: these elements constitute, according to the principle, the most just social alternatives. We will refer to the maximal elements of the various relations of justice as the outcomes, or the solutions, of the corresponding principles. The set of maximal elements will be denoted by one- or two-letter symbols corresponding to the name of the relevant principle.

Considering the vector of distances in Table 2, two properties of what one may want to see as a just

TABLE 2

Distances of All 13 Social Orders from Jurors' Preference Orders in Profile I, Percentage of Choices Observed in the Polish Study, and the Best Social Orders Prescribed by Principles of Justice

SOCIAL ORDER	DISTANCES FROM JURORS' PREFERENCE ORDER			ORDERED VECTORS OF DISTANCES	PERCENTAGE OF SUBJECTS CHOOSING	BEST WITH RESPECT TO WHAT PRINCIPLES OF JUSTICE	
	ABC	BCA	CBA			STANDARD	GENERALIZED
ABC	0	4	6	(0, 4, 6)	5.7	**MC**	C1
BCA	4	0	2	(0, 2, 4)	55.7	**U, MC, LC**	R2, R3, C1, C2, C3
CAB	4	4	2	(2, 4, 4)	2.1	—	—
ACB	2	6	4	(2, 4, 6)	.7	—	—
BAC	2	2	4	(2, 2, 4)	.7	—	R2
CBA	6	2	0	(0, 2, 6)	13.6	**MC**	C1, C2
A–BC	1	3	5	(1, 3, 5)	.7	—	—
A–CB	3	5	3	(3, 3, 5)	.0	—	—
B–CA	5	1	1	(1, 1, 5)	10.0	—	R2, R3, C2, C3
AB–C	1	5	5	(1, 5, 5)	.0	—	—
BA–C	3	1	3	(1, 3, 3)	5.0	**MR, LR**	R1, R2, R3, C3
CA–B	5	3	1	(1, 3, 5)	2.1	—	—
A–B–C	3	3	3	(3, 3, 3)	3.6	**E, RE, MR**	R1, R2

Note: Total number of valid cases N = 140. Orders chosen by the various principles of justice are identified as **E** if an order is a solution of the egalitarian principle, **RE** if radical egalitarian, **U** if utilitarian, **LR** if Rawls' leximin, **MR** if Rawls' maximin, **LC** if conservative leximax, **MC** if conservative maximax; R1, R2, and R3 if first-, second-, and third-degree Rawlsian, respectively; and C1, C2, and C3 if first-, second-, and third-degree conservative, respectively.

outcome come naturally to mind: on one hand we may like the outcome to maximize the sum of individual utilities, and on the other, we may want it to minimize some measure of variance among individual utilities. Both properties would have to assume cardinal-level utilities—assumption 1A. The first three standard principles of justice, to be discussed, are simple operationalizations of these two intuitions.

The *utilitarian* (**U**) principle evaluates a social alternative by taking a sum of its utility for all individuals. The distribution or distributions that have the maximal sum are considered the best, and these are the utilitarian solutions. We will denote their set by **U**. If we look at the column of distances in Table 2, it is easy to see (keeping in mind that distances are inversely related to utilities, so that the larger the distance, the smaller the utility) that the utilitarian solution in profile I is BCA. We denote this as **U** = {(0, 2, 4)}. This and all other vectors used in the examples are ordered vectors of distances.

The *radical egalitarian* (**RE**) principle ranks social alternatives according to the variance of individual utilities: the smaller the variance, the better the distribution. In profile I, A–B–C is the radical egalitarian choice, since it yields a distribution with the minimal variance of 0. A–B–C is, in addition, the only radical egalitarian solution in the first profile. There are cases, however, where two or more social alternatives have the same minimal variance. In such situations, the radical egalitarian principle may be further refined through a more demanding condition known as the egalitarian principle.

The *egalitarian* (**E**) principle also ranks social alternatives according to the variance of utilities, but in the case of equal variances, it requires that we choose the distribution(s) with the largest sum of utilities.[13] It so happens that in the example of profile I, the radical egalitarian and the egalitarian principle choose the same solution. This need not be the case with other profiles. In profile IV, for example, there are three radical egalitarian solutions: CAB (4, 4, 4), BAC (2, 2, 2), and A–B–C (3, 3, 3); and only one of them BAC (2, 2, 2), is the egalitarian choice. Naturally, each egalitarian solution has to be a radical egalitarian solution, which means that the set of all **E** alternatives is a subset of all **RE** alternatives: $\mathbf{E} \subsetneq \mathbf{RE}$.

All the above principles of justice required the strong condition of cardinal utilities—assumption 1A; the principles to be discussed next use a much weaker, ordinal type of utilities and hence require assumption 1B. We begin with two standard Rawlsian principles—Rawls' maximin and leximin.

The rule behind the *Rawls' maximin* (**MR**) is to maximize the utility of the worst-off person; in our case, it would be the juror(s) whose preference is the most distant one from the social choice order. In profile I (Table 2), there are two social alternatives BA–C (1, 3, 3) and A–B–C (3, 3, 3), where the maximal distance (3) is smaller than the maximal distance in other social alternatives. This means that BA–C and A–B–C maximize the utility of the worst-off juror and thus constitute Rawls' maximin solutions. Formally, maximin does not differentiate between (1, 3, 3) and (3, 3, 3), even though the first solution, which is equally good for two jurors and better for one, may otherwise seem superior. This intuition motivates, however, a more demanding rule of the Rawls' leximin principle (Rawls 1971, 1974).

Formally, *Rawls' leximin* (**LR**) is a lexicographic extension of the Rawls' maximin, which provides a

TABLE 3

Choices in the Polish Study and the Outcomes of the Classical Principles of Justice

	PRINCIPLES REQUIRING INTERVAL MEASUREMENT			PRINCIPLES REQUIRING ORDINAL MEASUREMENT			
PROFILE	RADICAL EGALITARIAN	EGALITARIAN	UTILITARIAN	RAWLS' MAXIMIM	RAWLS' LEXIMIN	CONSERVATIVE MAXIMAX	CONSERVATIVE LEXIMAX
ABC I BCA CBA	A–B–C	A–B–C	BCA	BA–C, A–B–C	BA–C	ABC, BCA, CBA	BCA
ABC II BCA ACB	A–B–C	A–B–C	ABC	A–BC, A–B–C	A–BC	ABC, BCA, ACB	ABC
ABC III BCA BAC	A–B–C	A–B–C	BAC	BAC	BAC	ABC, BCA, BAC	BAC
ABC IV ABC BCA	CAB, BAC, A–B–C	BAC	ABC	BAC	BAC	ABC, BCA	ABC
ABC V ABC CAB	BCA, ACB, A–B–C	ACB	ABC	ACB	ACB	ABC, CAB	ABC
ABC VI ABC CBA	A–CB, BA–C, A–B–C	A–CB, BA–C, A–B–C	ABC	A–CB, BA–C, A–B–C	A–CB, BA–C, A–B–C	ABC, CBA	ABC
ABC VII ABC ACB	B–CA, AB–C, A–B–C	AB–C	ABC	AB–C	AB–C	ABC, ACB	ABC
ABC VIII ABC BAC	A–BC, CA–B, A–B–C	A–BC	ABC	A–BC	A–BC	ABC, BAC	ABC
Percentage of Subjects Choosing[a]	0 (0)	0 (0)	4.3 (6)	0 (0)	0 (0)	27.1 (39)	4.3 (6)

Note: Total number of valid cases N = 140.

[a]Numbers of cases are in parentheses.

refinement for cases when the worst-off persons are equally well off in two or more different distributions; in these cases, **LR** prescribes to maximize the utility of the second-worst-off person, and if these are equal, the third-worst-off person, and so on. In profile I, BA–C is the only Rawls' leximin solution. Again, each leximin solution is also a maximin solution, and so, **LR** ⊆ **MR**.

Since Rawlsian choices are based on the utility levels of the worst-off individuals, it is only natural to consider a complementary set of principles based on maximizing the utility levels of the best-off individuals (see Kern 1978). Such principles are, quite understandably, called conservative. The conservative counterpart to the Rawls' maximin, the *conservative maximax* (**MC**) principle, requires maximizing the utility of the best-off person and the *conservative leximax* (**LC**) principle is defined as the lexicographic extension of **MC**. In our case, orders that maximize the utility of the best-off person are simply those which are identical to one or more of jurors' own preferences. In the example of Profile I (Table 2), the conservative maximax solutions are ABC (0, 4, 6), BCA (0, 2, 4), and CBA (0, 2, 6); BCA is the only conservative leximax outcome. As before, each leximax solution is also a maximax solution and so, **LC** ⊆ **MC**.

Table 3 examines the fit between the empirical distributions and the outcomes of the standard principles of justice. Before we look at the results, it is important to realize that, as opposed to the social welfare functions, principles of justice can generate more than one solution in a single profile. In consequence, we may have many different distributions that agree with solutions of a single principle. The radical egalitarian principle, for instance, explains 243 (1 × 1 × 1 × 3 × 3 × 3 × 3 × 3) different distributions (3 of them are also explained by **E**), **MR** explains 12 (3 of them explained by **LR**), and **MC** explains 864 (1 explained by **LC**). Given that different principles of justice are not independent (e.g., in our case **U** = **LC**), the total number of different distributions explained by these principles is 243 + 12 + 864 = 1,119. As we recall that the social welfare functions can explain 3 different distributions, and the 3 are consistent with 18.5% of the data, we may expect that the large increase in the sheer number of theoretically possible distributions will generate a sizable improvement in the percentage of data explained. It does not. Only 27.1% of the empirical distributions agree with the 1,119 theoretical ones. In particular, no empirical distribution agrees with any of the egalitarian or Rawlsian principles, and it is only the utilitarian and conservative principles that account for the 27.1% explained.

In addition to the standard principles of justice, we may want to consider rules that are mixtures of the standard principles—agree with some on some profiles, and others on other profiles. This will be of value for determining a normative dimension of choice only if we are actually able to find some normative property behind any such mixed rule. To see if this course of action is justified, we ought to look at each profile separately (see Table 4). In the two worst cases of profiles V and IV, we find that 40% and 32.1% of choices remain unexplained. This is enough to conclude that standard principles of justice, either mixed or pure, cannot provide a sufficient explanation.

Finally, there is one more far-fetched and obscure possibility that some normative justification can be obtained through a mixture of the standard principles of justice and the social welfare functions. To assess such a possibility (normative and other justifications aside), we ought to look at the combined explanatory power of the social welfare functions and the principles of justice. Even when joined, as it turns out, the two classes do not predict well. They fail to account for 58.6% of empirical distributions and up to 28.5% (profile II) in individual profiles (see Table 4). Like each of them separately, the social welfare functions combined with the principles of justice remain insufficient to explain this data.

THE EMPIRICAL RELEVANCE OF THE THEORETICAL FRAMEWORK

Standard principles of justice have clearly failed to explain the data, and some exploration of possible reasons is more than appropriate, especially since the theoretical framework involved has a number of components and not all of them may be responsible for the observed lack of fit. The most fundamental element of the framework, no doubt, is the distance function: if the distance measure is not related to the actual choices, we may not get a satisfactory fit regardless of other components of a formal description. The goal of this section is to verify the empirical relevance of the basic components of our theoretical framework. Figure 2 will serve us as the main tool in reaching this objective.

Figure 2 displays all distributions of choices made by subjects in the study. Since its form is both complex and nonstandard, we begin with an extended comment on its content. Figure 2 consists of eight graphs related to the eight profiles used in the experiment. Each graph has the same basic structure: it is a map of distances on a set of all possible (weak) orders of three alternatives. A number next to each order indicates the percentage of subjects who have chosen it in the study. This way, the eight graphs include the full distribution of data. Finally, jurors' preferences (the element that defines a profile) are marked by arrows. Relative positions of these arrows give us a reading on the amount of discordance in a profile. Profiles I, II, and VI, for instance, show more discordance (have larger distances between juror's preferences) than profiles VII and VIII. (Note that the larger the discordance in a profile, the larger the spread in the distribution of choices.)

With Figure 2 at hand, we can pose the problem of the relevance of the distance framework. Perhaps the most striking geometric regularity in the eight graphs of Figure 2 is that almost all of our data falls somewhere in between the two most distant arrows (preferences of the jurors). This geometric regularity needs to be explained, however, by some normative conditions of choice. Much of the discussion to follow builds on two standard notions of Pareto optimality: weak and strong. These two notions, in contrast to their standard usage, will be applied here to ordered vectors (i.e., vectors whose coordinates are ordered from the smallest to the largest one).

The Condition of Weak Pareto Optimality for Ordered Vectors. We will say that an ordered vector $(d_1, \ldots, d_n)$ is better in the weak Pareto sense than $(d_1^*, \ldots, d_n^*)$ if and only if $d_i < d_i^*$, for all $i = 1, \ldots, n$.

A vector that is better in the weak Pareto sense has smaller distances or, equivalently, larger utilities, in all coordinates. If we were looking for the best solution, it might certainly be the minimal optimality condition to require that our choice is not worse, in this sense, than some other vector in the set—or, as we would equivalently say, that our choice is not weakly dominated. In Figure 2, choices that are not weakly dominated are marked by both black and shaded nodes. It is easy to see that these weakly optimal orders while explaining almost all observed choices also explain orders that have not been chosen by anyone (see, e.g., profile VI). And so, since weak optimality looks like a necessary, though not a sufficient, component of a theory that would successfully explain the data, it may be useful to consider next a more stringent notion of optimality.

The Condition of Strong Pareto Optimality for Ordered Vectors. We will say that an ordered vector $(d_1, \ldots, d_n)$ is better than, or dominates, $(d_1^*, \ldots, d_n^*)$ in the strong Pareto sense if and only if $d_i \leq d_i^*$, for all $i = 1, \ldots, n$ and for at least one j, $d_j < d_j^*$.

A vector that is better in the strong Pareto sense has all distances smaller than or equal to, and at least one of them strictly smaller. (Consequently, of course, the utilities in the better vector are all larger than or equal to, and at least one is larger.) We denote this relation by $\succ$ and call it a *strong Pareto relation.* Orders that are not dominated by any other orders in the set are called optimal in the strong Pareto sense or, in short, strongly optimal.

As we recall that the Suppes' solutions were defined as strongly optimal elements of the set of ordered vectors, it becomes obvious that contrary to the weak Pareto optimality, the strong condition has a clear normative justification. The stronger condition is, naturally, a more demanding one so that every strongly optimal is also a weakly optimal solution but not vice versa. Strongly optimal orders are marked in Figure 2 with black nodes and these are clearly central to the observed distributions of choices. This, in addition to the normative significance of strongly optimal orders, indicates that they ought to be considered as a core, or a necessary element, of any descriptively adequate theory. It is also clear, when looking at Figure 2, that the strongly optimal orders are not sufficient to explain the choices. They provide no more than a lower approximation theory. The case of profile II, where 43.6% of choices are not strongly optimal, is sufficient to prove the point.

Hence, so far, our analysis of the data leads to two types of conclusions. First, the analysis confirms, either directly or indirectly, the relevance of the crucial components of the theoretical framework: the Kemeny–Snell distance measure, the assumed relations between distances and utilities, and the assumption of impartiality. (Note that since the criteria of Pareto optimality assume only ordinal measure of utilities, assumption 1B is sufficient for this analysis.) Second, this confirmation ultimately points to the direction of the subsequent analysis. The bulk of the data behaves very regularly: all but few outlying choices are weakly optimal, while the strongly optimal orders lie in the center of the distributions. The weak and strong optimality conditions provide thus upper and lower approximation theories: a successful solution has to lie somewhere in between.

THE GENERALIZED RAWLSIAN AND CONSERVATIVE PRINCIPLES OF JUSTICE

The lack of fit between the data and the strongly optimal orders should not be surprising: solutions considered to be just need not, after all, be strongly optimal. In the example of profile I (Table 2), for instance, the conservative maximax CBA (chosen by 13.6%) and two Rawls' maximins ABC and A–B–C (chosen by 5.7% and 3.6%, respectively) are not strongly optimal; and naturally, there is no good reason to expect that either maximin or maximax (except for their lexicographic versions) would be. Both principles are, after all, concerned with solution's optimality for a specific person—the worst-off and the best-off individual, respectively—not its optimality for all.

Yet both maximax and maximin can be seen as strongly optimal in a different sense. Formally, we can say that they are strongly optimal in a domain that consists of vectors composed of only one coordinate of the ordered vector—the first one for maximax and the last one for maximin. Take, for instance, the case of Rawls' maximin. A relation $\succ_{R1}$ on a set of ordered vectors that leads to the maximin solution can be formally defined as follows:

$$(d_1, \ldots, d_n) \succ_{R1} (d_1^*, \ldots, d_n^*) \textit{ iff } (d_n) \succ (d_n^*).$$

$R1$, the set of the optimal elements of $\succ_{R1}$ is equal to **MR**—the set of Rawls' maximin solutions. The conservative maximax can be similarly defined by the following relation $\succ_{C1}$:

$$(d_1, \ldots, d_n) \succ_{C1} (d_1^*, \ldots, d_n^*) \textit{ iff } (d_1) \succ (d_1^*).$$

C1, the set of the optimal elements of $\succ_{C1}$, is equal to the set of conservative maximax solutions **MC**.

An important advantage of this otherwise cumbersome formal characterization is that it extends naturally to a simple generalization of both principles. Since maximin selects a solution that is strongly Pareto optimal for the *single* worst-off individual, it is only natural to ask about a solution that would maximize, in the same strong Pareto sense, the utility of *two, or three, or any k* worst-off individuals in the society. And so, we have the following generalization of $\succ_{R1}$ to $\succ_{Rk}$, for any $k = 1, \ldots, n$:

$$(d_1, \ldots, d_n) \succ_{Rk} (d_1^*, \ldots, d_n^*) \textit{ iff } (d_{n-k+1}, \ldots, d_n) \succ (d_{n-k+1}^*, \ldots, d_n^*).$$

The $\succ_{Rk}$ relation defines what we call *the generalized* k*th degree Rawlsian relation of justice*. We will denote the set of optimal elements of $\succ_{Rk}$, that is, vectors that are strongly Pareto optimal in the last k coordinates, as Rk. An ordered vector is kth degree Rawlsian if no other ordered vector has the last k coordinates that are smaller than or equal to and at least one is strictly smaller.

The same rationale can now be applied to generalize the standard conservative solutions. Here, we define *the* k*th degree conservative* solution as a vector that is strongly Pareto optimal for the k best-off individuals in the society, that is, the optimal element of the following *generalized* k*th degree conservative relation of justice* $\succ_{Ck}$, for $k = 1, \ldots, n$:

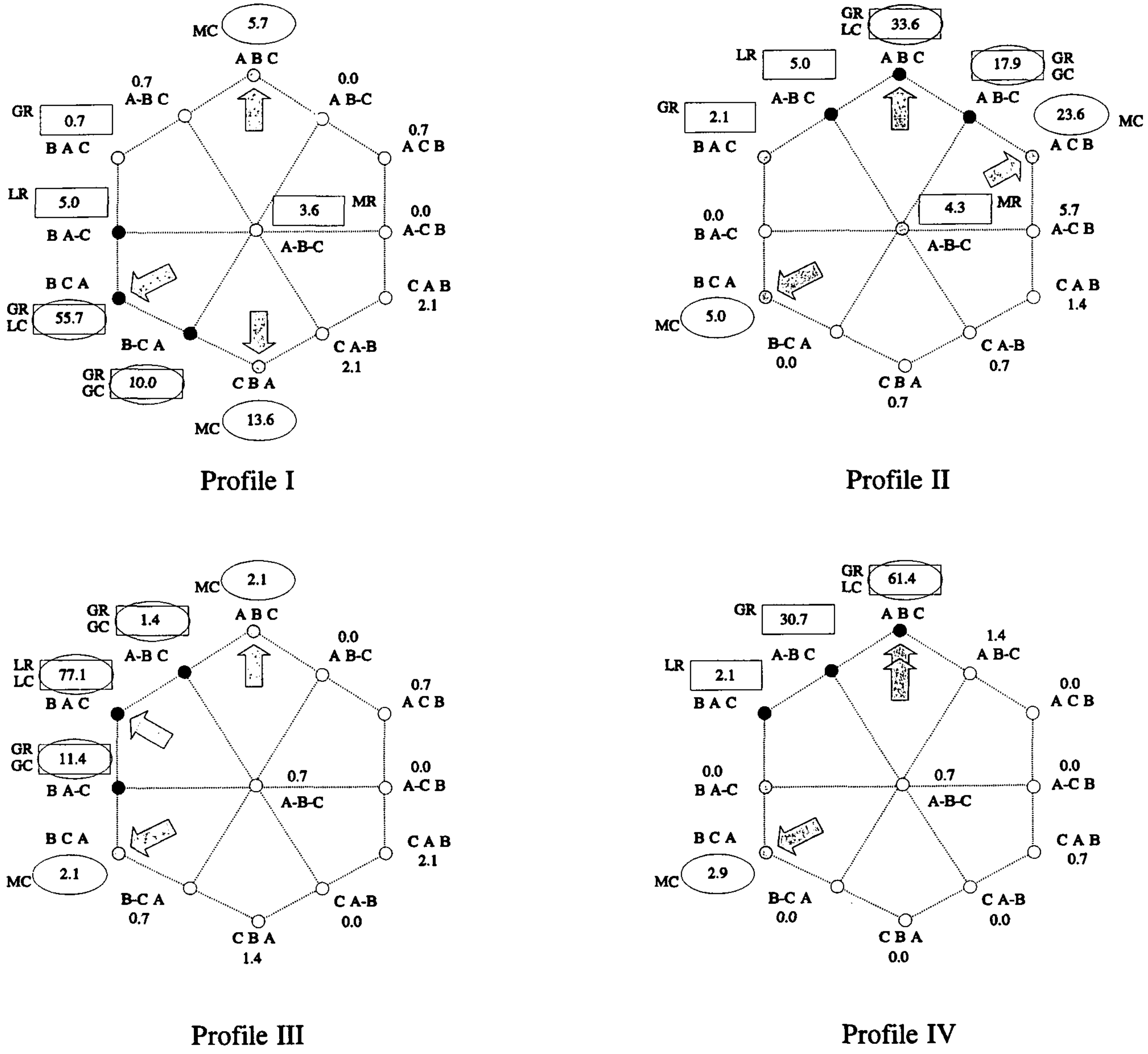
$$(d_1, \ldots, d_n) \succ_{Ck} (d_1^*, \ldots, d_n^*) \textit{ iff } (d_1, \ldots, d_k) \succ (d_1^*, \ldots, d_k^*).$$

We will denote the set of the kth degree conservative solutions by Ck. An ordered vector is kth degree conservative if no other ordered vector has the first k coordinates that are smaller than or equal to and at least one is strictly smaller.

The first important—though rather evident—fact to note is that generalized principles do not depend on

FIGURE 2

Distribution of Choices (in %) in the Polish Study and Their Analysis in the Framework of the Generalized Principles of Justice


MC 5.7
A B C
0.7 A-B C
0.0 A B-C
GR 0.7 B A C
0.7 A C B
LR 5.0
3.6 MR
B A-C
0.0 A-C B
A-B-C
B C A
C A B 2.1
GR LC 55.7
B-C A
C A-B 2.1
GR GC 10.0
C B A
MC 13.6
GR LC 33.6
LR 5.0
A B C
17.9 GR GC
GR 2.1
A-B C
A B-C
23.6 MC
B A C
A C B
0.0 B A-C
4.3 MR
5.7 A-C B
A-B-C
B C A
C A B 1.4
MC 5.0
B-C A 0.0
C A-B 0.7
C B A 0.7
MC 2.1
GR GC 1.4
A B C
0.0 A B-C
LR LC 77.1
A-B C
0.7 A C B
B A C
GR GC 11.4
B A-C
0.7
0.0 A-C B
A-B-C
B C A
C A B 2.1
MC 2.1
B-C A 0.7
C A-B 0.0
C B A 1.4
GR LC 61.4
GR 30.7
A B C
1.4 A B-C
LR 2.1
A-B C
B A C
0.0 A C B
0.0 B A-C
0.7
0.0 A-C B
A-B-C
B C A
C A B 0.7
MC 2.9
B-C A 0.0
C A-B 0.0
C B A 0.0

Profile I

Profile II

Profile III

Profile IV

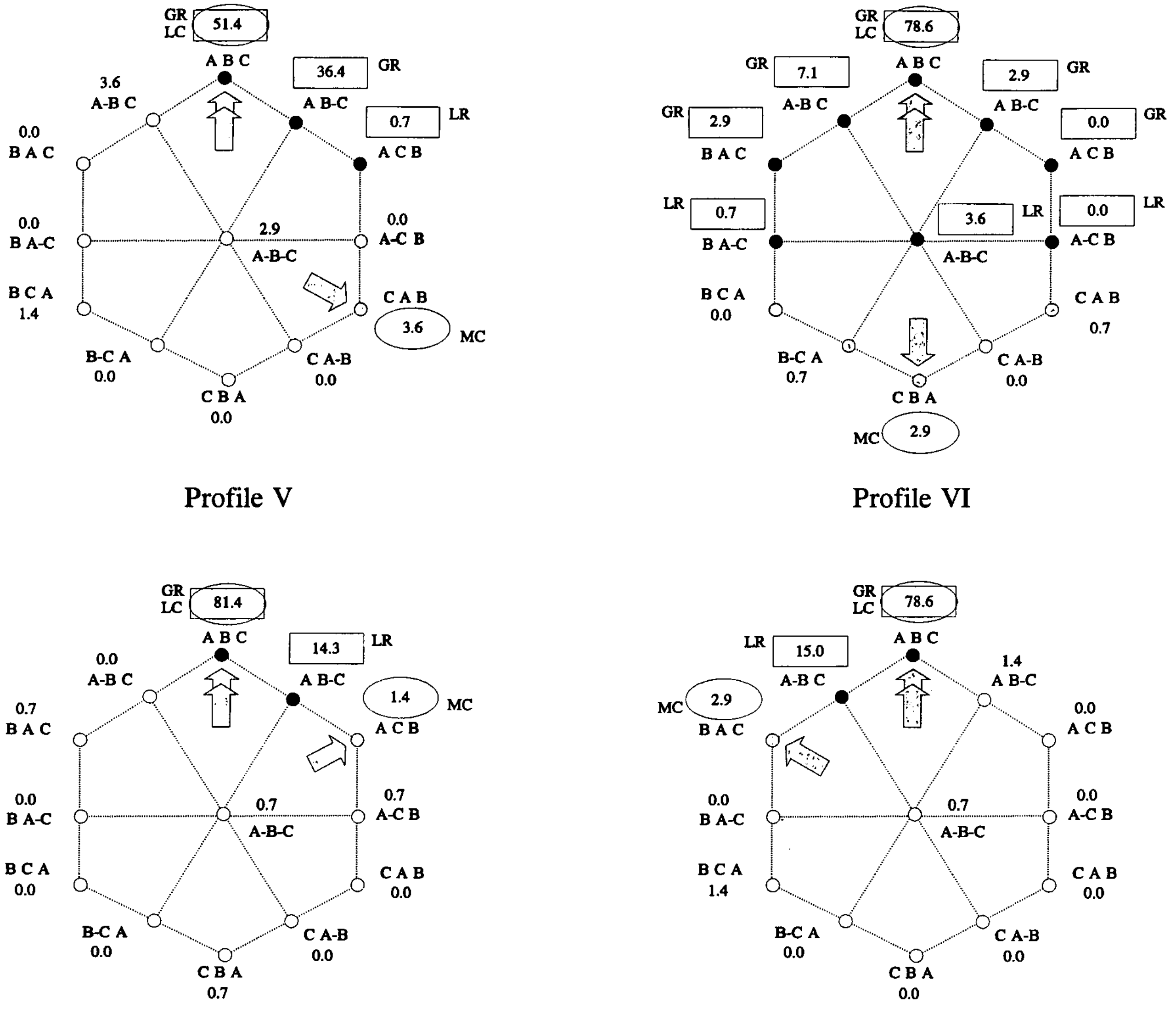
the specific context of orders and distances from which they have been derived here. The principles apply to any distributive justice problem, and they apply under the same conditions required for the standard Rawlsian and conservative solutions: these include the ordinal measurement of utilities and the condition of impartiality. To illustrate the generalized solutions, we have identified all *k*th degree Rawlsian and conservative solutions in the example of profile I in Table 2.

The rationale behind the proposed generalizations of the standard Rawlsian and conservative principles of justice is essentially the same as the one behind the standard solutions and does not need an additional comment. What may not be evident, however, are some fundamental properties of these solutions. We state them below as propositions 1–4. The first two propositions speak to the optimality of the generalized solutions, and the last two show how different classes of solutions are related to each other. All proofs are given in the Appendix.

PROPOSITION 1. *For all* k = 1, . . . , n *the* kth *degree Rawlsian and conservative solutions are weakly Pareto optimal in the set of ordered vectors.*

Consequently, the weak optimality that (as we have noted earlier) is the property of the standard Rawlsian and conservative solutions applies to the whole class of generalized solutions. These solutions are all optimal in this minimal sense. Hence, in the

FIGURE 2 continued


GR
LC
51.4
A B C
36.4
GR
A B-C
0.7
LR
A C B
3.6
A-B C
0.0
B A C
0.0
B A-C
2.9
A-B-C
0.0
A-C B
B C A
1.4
C A B
3.6
MC
B-C A
0.0
C A-B
0.0
C B A
0.0
GR
LC
78.6
GR
7.1
A-B C
A B C
2.9
GR
A B-C
GR
2.9
B A C
0.0
GR
A C B
LR
0.7
B A-C
3.6
LR
A-B-C
0.0
LR
A-C B
B C A
0.0
C A B
0.7
B-C A
0.7
C A-B
0.0
C B A
MC
2.9
GR
LC
81.4
A B C
14.3
LR
A B-C
1.4
MC
A C B
0.0
A-B C
0.7
B A C
0.0
B A-C
0.7
A-B-C
0.7
A-C B
B C A
0.0
C A B
0.0
B-C A
0.0
C A-B
0.0
C B A
0.7
GR
LC
78.6
LR
15.0
A-B C
A B C
1.4
A B-C
0.0
A C B
MC
2.9
B A C
0.0
B A-C
0.7
A-B-C
0.0
A-C B
B C A
1.4
C A B
0.0
B-C A
0.0
C A-B
0.0
C B A
0.0

Profile V

Profile VI

Profile VII

Profile VIII

⇨ individual preference, element of a profile

● strongly Pareto optimal (grading principle) choices

◌ weakly Pareto optimal choices (which are not strongly optimal)

○ neither strongly nor weakly optimal choices

▭ Rawlsian solutions of all types
LR Rawls leximin (LR=**LR**)
MR Rawls maximin (MR=**MR-LR**)
GR generalized Rawlsian (GR=**GR-MR**)

⬭ conservative solutions of all types
LC conservative leximax (LC=**LC**)
MC conservative maximax (MC=**MC-LC**)
GC generalized conservative (GC=**GC-MC**)

situation of a conflict between equality and optimality—the central dilemma for all concepts of justice—the generalized solutions are guided by the concerns of optimality. No such correspondence exists, however, between the generalized solutions and the strong Pareto optimality condition. Here the relationship is much weaker and similar, again, to the case of the standard principles.

PROPOSITION 2. *For a set of ordered vectors of length* $n > 1$ *and for any* $k = 1, \ldots, n - 1$,

> *i. a* kth *degree Rawlsian or conservative solution does not have to be strongly Pareto optimal.*
>
> *ii. a strongly optimal vector does not have to be a* kth *degree Rawlsian or conservative solution.*

Proposition 2 is, in fact, a special case of a more general theorem: conditions of strong Pareto optimality and optimality in the sense of $>_{Rk}$ or $>_{Ck}$, for $k = 1, \ldots, n - 1$, can be shown to be independent: neither the presence nor the absence of either one of them implies the presence or the absence of the other. What is more, however, independence holds for other sets of solutions as well. In general, we have the following theorem.

PROPOSITION 3. *With the only exception of* $\{Rn, Cn\}$, *any two elements of* $\{R1, \ldots, Rn, C1, \ldots, Cn\}$ *are independent.*

Proposition 3 essentially tells us that there is no redundancy among the classes of the generalized solutions—vectors that are optimal in terms of one of the generalized relations may or may not be optimal in terms of the others.[14] This statement would have been true of any two solution sets, if it was not for the singular case of $\{Rn, Cn\}$. These two sets cannot be independent, for an obvious reason that we formulate as the next proposition.

PROPOSITION 4. $Rn = Cn$.

Equality of Rn and Cn follows directly from the definitions of $>_{Rn}$ and $>_{Cn}$ and is formally trivial to prove. Some of its consequences are important, however. Rn and Cn are identical because they are both equal to the set of strongly optimal vectors (in the set of ordered vectors), and this makes them equal, by definition, to the set of vectors that are optimal under the Suppes' relation of justice. Yet it is somewhat inappropriate to refer to Rn as Rawlsian and Cn as conservative solutions. They are essentially neither. The very idea behind the Rawlsian solution is to maximize the utility of the worst-off—either individual, in case of the standard principles, or a group, in the generalized version. When the group turns out to be the whole society, the very meaning of the predicate *Rawlsian* is lost; all that remains is strong Pareto optimality of the solution. The same can be said, of course, about the conservative solution Cn. It only makes sense, then, to partition solutions into the following three classes:

1. The Generalized Rawlsian Solutions: $\mathbf{GR} = R1 \cup R2 \cup \ldots \cup R(n - 1)$
2. The Generalized Conservative Solutions: $\mathbf{GC} = C1 \cup C2 \cup \ldots \cup C(n - 1)$
3. The Strongly Pareto Optimal Solutions (Suppes' Solutions): $\mathbf{S} = Rn = Cn$

Hence, the set of the generalized Rawlsian solutions consists of all Rawlsian solutions of degree $n - 1$ or smaller. All outcomes in this set are best for some k worst-off individuals in the society, where k denotes the size of the group ($k = 1, \ldots, n - 1$). The generalized Rawlsian solutions are thus a natural extension of Rawls' maximin that is optimal for the single worst-off individual. Moreover, the standard Rawlsian principles of justice are special cases of the generalized solutions. More specifically, $\mathbf{MR} = R1$, $\mathbf{LR} = R1 \cap R2 \cap \ldots \cap Rn$, and $\mathbf{LR} \subseteq \mathbf{MR} \subseteq \mathbf{GR}$.

Analogously, the set of the generalized conservative solutions consists of all conservative solutions of degree $n - 1$ or smaller. These solutions select outcomes that are best for the k ($k = 1, \ldots, n - 1$) best-off members of the society. Hence, they naturally generalize conservative maximax—the optimal solution for the single best-off individual in the society. Again, the standard conservative principles are special cases of the generalized solutions. More specifically, $\mathbf{MC} = C1$, $\mathbf{LC} = C1 \cap C2 \cap \ldots \cap Cn$, and $\mathbf{LC} \subseteq \mathbf{MC} \subseteq \mathbf{GC}$.

Suppes' solutions, finally, select outcomes that are "best" (in the strongly optimal sense) for the whole society. Recall that under Suppes' grading principle of justice, one outcome is more just than another if it gives no less utility to every person, and strictly more, to at least one, than someone in the corresponding position obtains under the second outcome. This means that Suppes' solutions improve the outcome for the entire group; any specific person in the group may either lose or gain as the group switches to a Suppes' outcome.

The three categories of justice correspond in a natural way to the notions of the left, the center, and the right in politics: Rawlsian solutions are optimal for a group of the worst-off members of the society; the conservative, for a group of the best-off; and the strongly optimal solutions are not biased in either direction and, in this sense, remain central.

EMPIRICAL CHOICES AND THE GENERALIZED PRINCIPLES OF JUSTICE

In this section we will use the three generalized principles of justice—Rawlsian, conservative, and strongly optimal—to analyze choices made by subjects in the study. Distributions of choices in all profiles can be seen in Figure 2. Percentages of subjects who chose orders that fall in the general Rawlsian category (**GR**) are framed by rectangles, and percentages that fall in the conservative category (**GC**) are framed by ellipses. Moreover, since catego-

TABLE 4

Explanatory Power of Various Frameworks, the Polish Study

FRAMEWORK	PERCENTAGE OF CHOICES REMAINING UNEXPLAINED IN PROFILES I	II	III	IV	V	VI	VII	VIII	PERCENTAGE OF EMPIRICAL DISTRIBUTIONS REMAINING UNEXPLAINED ACROSS PROFILES
Social welfare functions	44.2	66.4	22.6	7.8	12.2	21.5	18.5	21.4	80.0
Standard principles of justice	16.3	28.5	17.7	32.1	40.0	14.3	2.1	2.8	68.6
Social welfare functions or standard principles of justice	16.3	28.5	17.7	1.4	3.6	14.3	2.1	2.8	47.1
Generalized principles of justice	5.6	8.5	5.6	2.8	7.9	1.4	2.8	3.5	19.3

Note: Total number of valid cases N = 140. We consider an empirical distributions as being explained by social welfare functions if it agrees with some standard social welfare function, not necessarily the same one in all profiles. The same holds for percentages explained by other frameworks.

ries of solutions form descending sequences of sets, **GR** ⊃ **MR** ⊃ **LR** and **GC** ⊃ **MC** ⊃ **LC**, it is good to identify, for each generalized solution, the lowest level category a solution belongs to. We will distinguish these exclusive categories from the inclusive ones by using the regular type for the first category and the bold type for the second. And so, we will write *GR* to denote Rawls' generalized solutions that are not maximin (*GR* = **GR** − **MR**), *MR* to denote Rawls' maximin that are not leximin (*MR* = **MR** − **LR**), and *LR* to denote Rawls' leximin (*LR* = **LR**). Similarly, in the class of conservative solutions, we will use *GC* for generalized conservative solutions that are not maximax (*GC* = **GC** − **MC**), *MC* for conservative maximax that are not leximax (*MC* = **MC** − **LC**), and *LC* for conservative leximax (*LC* = **LC**). (This last distinction is, of course, redundant: we make it to preserve notational consistency.) With this notation in mind, we can now return to the figure. Even a quick inspection of the distribution of choices conveys a fairly good idea of the explanatory power of the theoretical framework. The degree of fit is quite remarkable: the generalized solutions account for 95.2% of all choices made by the subjects.

Table 4 lists percentages of choices unexplained by the generalized solutions and compares them with the percentages obtained with the social welfare functions and the standard principles of justice—the improvement over the two frameworks, considered separately or jointly, is substantial. Notably, the key increase is obtained in profiles I, II, III, and VI, and these profiles are critical for the analysis: they all have a high degree of conflict among jurors' preferences and a large variance in the distribution of choices; and, in all of them, the standard solutions leave a great many choices unexplained.

As Table 4 compares the explanatory power of the three different frameworks, it will be good to know to what extent the increase in the percentage of choices predicted by the generalized solutions may simply be a function of an increase in the number of categories they predict. Interestingly, the generalized solutions explain 40.4% of all 104 (13 × 8) social choices, whereas the social welfare functions and the standard principles of justice (taken together) explain an almost identical fraction—37.5%. The difference between the two percentages turns out to be trivial as compared to the difference in their explanatory power.

In addition to explaining a sufficiently large part of data, a descriptively adequate theory should do one more thing: it should not predict categories that are empty. Hence, it is important to note that the generalized solutions are equally successful in avoiding the problem of overprediction: while 36 potential social choices have not been chosen by anyone, only two of these choices, both in profile VI, were predicted by the generalized solutions. This translates into a mere 5.5% rate of overprediction.

The 95.2% choices explained on average within profiles would lead us to expect that the percentage of empirical distributions explained will be high as well. It is easy to see, for instance, that this percentage has to be somewhere between 61.9% and 91.5%. Moreover, if the 4.8% unexplained choices were distributed independently of the theoretical categories, we would only expect to see about two-thirds of the empirical distributions explained. The actual fit is higher than these expectations: the generalized solutions are consistent with 80.7% of the 140 empirical distributions.[15] More specifically, 80.7% of empirical distributions are explained by some generalized solution, not necessarily the same one in all profiles. This is clearly much higher than the analogous percentage explained by the standard social welfare functions (20.0%), standard principles of justice (31.4%), or both (52.9%).[16] In addition, we should note, the large percentage of empirical distributions explained is not a consequence of the set of theoretically predicted distributions being a large subset of the set of all possible distributions: distributions consistent with the generalized solutions constitute merely .04% of all possible distributions. Yet explaining the choices is only part of our endeavor. The ultimate goal of our analysis is to account for an ethical component of choice—a factor that would show consistently across decisions in all conflict situations—should a factor like this exist, naturally. Hence in the next step we ask: Are people indeed consistent, on some normative dimension, in what they judge to be a fair solution to various conflict situations? Is there an ethical constant in their choices?

A choice of dimensions on which we may expect subjects to be consistent ought to be a function of two factors: theoretical-normative and empirical-descriptive. At the theoretical normative level we may use any one of the solution categories. Yet the empirical-descriptive aspects will make most of the categories (*R*1, *R*2, etc) infeasible: the finer the normative dimensions we require, the larger the expected error in prediction. The prediction error will increase rapidly with the increase in the strength of constraints we impose on a theoretically predicted category. Hence, we propose to operationalize the problem of the normative consistency of choices as consistency within the general Rawlsian and conservative solution sets.

While the large size of the **GR** and **GC** sets provides security against errors in choice, the price we may have to pay for inflating categories within which we seek consistency is potentially high. The problem, as usual, is to avoid confusing an empirical phenomenon with some artifact of the formal construction. This may seem to be a strong possibility, especially since Figure 2 reveals a large overlap between Rawlsian and conservative categories. The percentage of choices that are classified as both **GR** and **GC** (framed in both rectangles and ellipses) varies from the minimum of 51.4% for profile V to the maximum of 89.4% for profile III; the average across profiles is 69.7%. Yet to see if consistency may indeed be a result of a large overlap we need to see what fraction of theoretical distributions is consistent with **GR** and **GC**; only if we were to find that this percentage is high could we then suspect a problem. This percentage is not high, however: only 7.6% of distributions are consistent with the generalized categories; a vast majority of them, 92.4%, are not.

Now, the stage is set to examine the problem of consistency. The following comparison of the observed percentage of empirical distributions (among those explained by the generalized solutions) consistent with **GR** and **GC** with the theoretical fractions gives the best picture of the degree of consistency observed in subjects' choices.

	Observed	Theoretical
Consistent	93.8	7.6
Inconsistent	6.2	92.4

We do not need statistical tests or measures of association to see that the relationship is very strong indeed.

The following, more detailed, decomposition accounts for the empirical distributions that are consistent with the **GR** and **GC** categories: **GR–GC**, 54.9%; **GC–GR**, 31.0%; **GR** ∩ **GC**, 7.9%.

Given the observed consistency of choices in the generalized Rawlsian and conservative categories and considering the explanatory power of this framework, we can use studies like this as a research tool to explore political attitudes. The general solutions **GR**, **GC**, and **S** can be used to account for the composition of attitudes within a group and to analyze their distribution across different groups.

CROSS-NATIONAL COMPARISONS: POLAND, THE UNITED STATES, AND JAPAN

When commenting on empirical studies of distributive justice, Sołtan writes, "In the research of Oppenheimer and Frohlich, . . . we can legitimately wonder to what degree the results are specific to a culture. . . . One way to answer these questions, of course, is to run the experiments in the most diverse cultural settings" (1987, 48). It would certainly be reasonable to expect that different societies may differ in dominant attitudes regarding the resolution of conflicts, whether these are problems of social choice or problems of distributive justice. In Poland, for example, where, for a number of decades, the official communist ideology had propagated radical egalitarianism, one might expect to find attitudes toward justice that would differ from these in the United States. Interestingly, Lissowski, Tyszka, and Okrasa (1991) showed that this need not be the case. Replicating experiments of Oppenheimer and Frohlich, they have actually demonstrated that attitudes towards justice in the two countries are remarkably similar. (In yet another study, a similar proximity of attitudes was observed between Poland and Israel [Lissowski 1993a].) Is it perhaps possible that these results indicate a more general tendency towards conflict resolution, one that would also show in the context of social choice? Would the "cultural settings" of Poland and the United States prove to be sufficiently diverse in the context of social choice to show any difference? And what if we were to add Japan to the set? How would attitudes in such diverse cultures as the United States, Poland, and Japan compare?

Two social choice studies that have been conducted independently of the Polish experiment will allow us to answer these questions. The original study, involving 70 subjects, was done in Japan (Tomiyama and Sayeki 1982). This study was replicated in 1988 by Swistak on 50 American students (see the Appendix). Using the three data sets, we will be able to compare the three distributions of Rawlsian and conservative choices. First however, we need to comment on one methodological problem that has consequences for the form and the accuracy of our conclusions.

The problem is that the three studies used different

TABLE 5

Results of Experiments in Poland, the United States, and Japan

		PERCENTAGE CHOOSING STRICT ORDERS					
PROFILE	COUNTRIES	ABC	BCA	CAB	ACB	BAC	CBA
ABC	Poland	6.6	71.2	2.5	.8	.9	18.0
I BCA	United States	.0	66.0	.0	.0	4.0	30.0
CBA	Japan	1.4	84.3	1.4	.0	2.9	10.0
		MC	LR, LC	MR		MR	MC
ABC	Poland	51.0	5.3	2.3	37.8	2.6	1.0
II BCA	United States	50.0	2.0	.0	44.0	4.0	.0
ACB	Japan	60.0	.0	10.0	15.7	14.3	.0
		LR, LC	MC	MR	MC	MR	
ABC	Poland	2.2	3.6	2.2	.7	89.9	1.4
III BCA	United States	.0	4.0	.0	.0	96.0	.0
BAC	Japan	.0	4.3	1.4	.0	94.3	.0
		MC	MC			LR, LC	
ABC	Poland	85.5	2.9	.7	.0	10.9	.0
IV ABC	United States	84.0	.0	2.0	.0	14.0	.0
BCA	Japan	35.7	1.4	.0	.0	62.9	.0
		GR, LC	MC			LR	
ABC	Poland	91.6	1.4	3.7	3.2	.1	.0
V ABC	United States	82.0	2.0	.0	16.0	.0	.0
CAB	Japan	38.6	.0	1.4	60.0	.0	.0
		GR, LC		MC	LR		
ABC	Poland	90.8	.0	.7	1.4	4.2	2.9
VI ABC	United States	76.0	.0	.0	8.0	16.0	.0
CBA	Japan	38.6	1.4	.0	20.0	38.6	1.4
		GR, LC	MR	MR	LR	LR	MC
ABC	Poland	96.2	.0	.0	2.4	.7	.7
VII ABC	United States	94.0	.0	.0	6.0	.0	.0
ACB	Japan	98.6	.0	.0	.0	.0	1.4
		LR, LC			MR, MC		
ABC	Poland	95.1	1.5	.0	.0	3.4	.0
VIII ABC	United States	96.0	2.0	2.0	.0	.0	.0
BAC	Japan	98.6	.0	.0	1.4	.0	.0
		LR, LC				MR, MC	

Note: Number of cases: 140 in the Polish study (transformed data), 50 in the American (see Appendix), and 70 in the Japanese (Tomiyama and Sayeki 1982). Orders are marked by the names of solution sets to which they belong (e.g., MC means that the corresponding order is a conservative maximax solution). Moreover, MC is the "exclusive category" (i.e., one that identifies only those conservative maximax solutions which are not leximax at the same time). The same applies to other categories. The full list of names and their abbreviations is given in the text and in Figure 1.

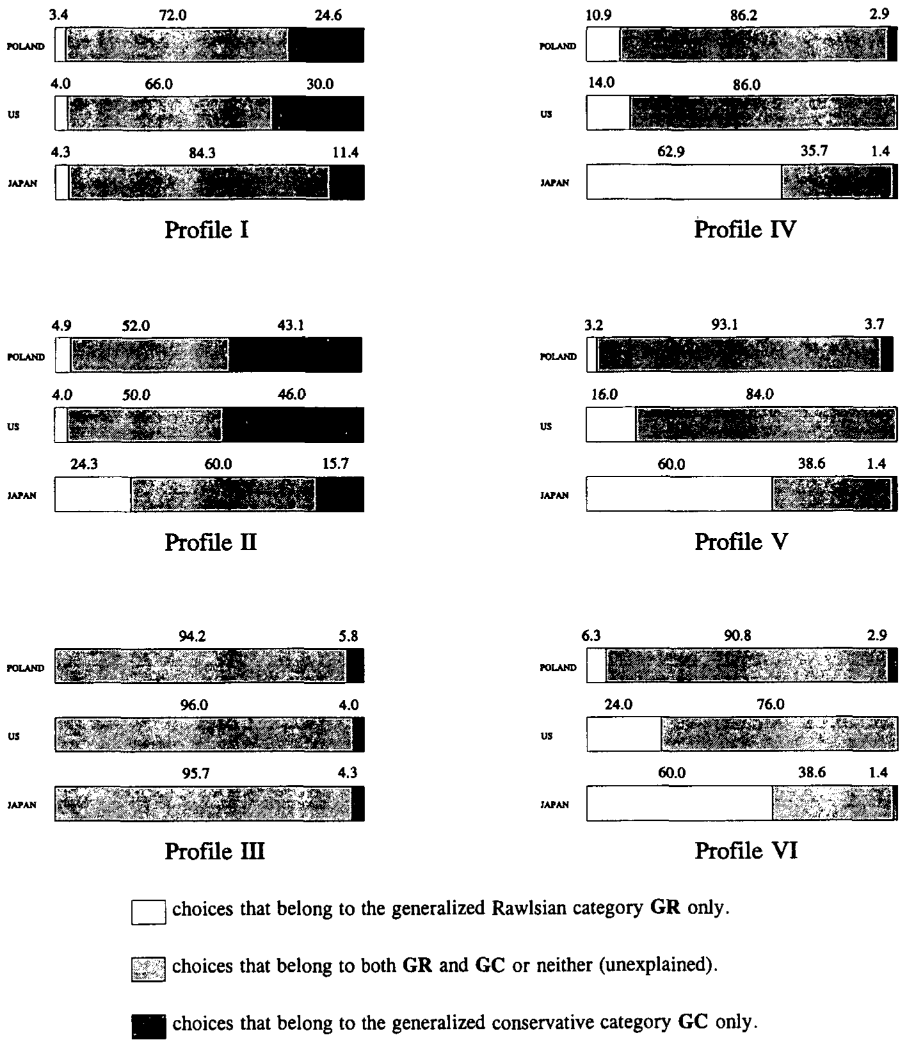
sets of social choices: in the Japanese and the American experiment, the set of orders from which subjects were asked to choose was constrained to six strict orders. To make the results of the three studies compatible, we have to transform the Polish data to the truncated form of six strict orders. One way to do this is to reassign choices that involve indifferences, (e.g., A–BC), to strict order categories. This would involve predicting which order a person who chose A–BC would have chosen had his options been limited to strict orders only: Would he have chosen ABC, or BAC, or yet another one? Although no such problem was anticipated when the Polish experiment was designed, some data obtained in a different part of the same study allowed us to predict these hypothetical choices.[17]

Table 5 shows the distribution of choices in Poland (transformed data), the United States, and Japan. Pairwise comparisons of all distributions, with a chi-squared test, confirm what can otherwise be clearly seen in the table. In profiles III, VII, and VIII, which have the least amount of conflict (small distances between jurors' preferences), there is no significant difference between any two countries (p-values $> .15$). Also in profile I, all tests are insignificant at .05 level. Differences show up in profiles II, IV, V, and VI, however. In all of them, we find highly significant differences between Japan and Poland and Japan and the United States (p-values $< .0007$). Yet again, there are no significant differences between the United States and Poland (at .05 level) with the one exception of profile VI (p-value $= .008$).

To interpret the nature of any differences, we first need a theory that explains the data; the theory, in our case, consists of the generalized principles of justice. Table 5 shows the explanatory power of this

FIGURE 3

Cross-national Comparisons: Poland, the United States, and Japan



framework: the smallest percentage of choices explained, across all profiles and all countries, is 95.7% (Poland in profile III). This almost perfect fit could have been expected; it is, in part, a result of the decrease in the number of social choice categories. The price we pay for truncating the set of social orders, however, is a much larger overlap between different solutions and, hence, more ambiguities related to possible interpretations. (The effect of using a smaller set of social orders is similar to the effect of using a less accurate scale of measurement: two categories that are distinct when measured accurately may become indistinguishable when measurement is less precise.)[18] One last qualification: in the Japanese and the U.S. cases, we do not have individual-level data. We cannot, thus, examine consistency of Rawlsian and conservative choices across profiles and, in consequence, we cannot assess the overall distribution of attitudes in these samples. Comparisons will have to be made for all profiles separately.

Comparisons of the generalized Rawlsian (**GR**) and conservative (**GC**) categories in Poland, the United States, and Japan are given in Figure 3. The figure, we should note, is simplified in the following three aspects: (1) the strongly Pareto optimal solution (Suppes's solutions) is not included, since it perfectly overlaps with the leximin or leximax, and with no individual-level data, it cannot be separated; (2) not all profiles are included (distributions in profiles III, VII, and VIII turned out to be all very similar, hence we chose to present profile III only); and (3) the unexplained choices are combined with the "middle category of orders for which Rawlsian and conservative choices overlap. (This last is a cosmetic simplification: since it is only meaningful to compare categories that do not overlap, the entire middle category is residual anyway.) The result of comparisons between the purely Rawlsian and purely conservative categories is clearly shown in Figure 3: while Poland and United States do not typically show differences, Japan stands out as having considerably more Rawlsian and considerably less conservative choices than either Poland or the United States.

In conclusion, our analysis confirms the observation that some culturally diverse societies may indeed differ in dominant attitudes regarding the resolution of conflicts like those in the problem of social choice. Certainly, a proper comparative analysis would require an extended series of fully compatible studies. Our results, partial as they are, demonstrate nevertheless that the theoretical framework developed in this paper provides a unique and very useful tool for such studies.

CONCLUSION

If there is a connection between voting methods used by groups and political attitudes held by their members, it would have been impossible to say what it is, in part because we do not have a theory that links social choices with attitudes that lead to them. These attitudes may, in many cases, be latent and deriving them indirectly from outcomes of peoples' judgments may be the only way to uncover their existence. It was our belief that the basic framework of the social choice theory could be used in an empirical study to identify these factors of normatively based decisions. Indeed, the simple content and the abstract form in which a social choice problem can be presented has led, as the analysis led us to believe, to a design where ethical-political attitudes could have formed the only basis for decision. Using the outcomes of these decisions we have conceptualized and separated the normative components of choice. On the way to this conclusion we have gone through a number of steps, including conceptual developments, deductive results, and data analysis. The logic of these developments and the basic findings are now summarized.

We began our analysis by comparing the choices observed in the study with the only normative solutions available to explain them—the outcomes of the standard social welfare functions. The fraction of choices that remained unexplained turned out to be very high: 81.4% empirical distributions and up to 66.4% of choices within profiles. The data strongly indicated that the issue of choice should be analyzed as a problem of distributive justice. But there are many ways to translate a social choice problem into a problem of distributive justice, each based on a different notion of distance. Fortunately, there is only one distance function, as we have shown, that satisfies normative conditions that ought to be required of a fair rule in this geometric representation. Standard principles of justice failed, however, to explain 72.9% of empirical distributions and up to 40.0% of choices within profiles. Yet when placed in the distance framework, the distribution of choices showed surprisingly strong regularities. This observation prompted us to explore some nonstandard solutions to the problem and, in consequence, led us to a new class of principles of justice. The new principles generalize the standard Rawlsian and conservative solutions; we explored their properties in propositions 1–4. The generalized solutions not only explained the data, they conceptualized two classes of solutions within which peoples' choices showed a very high consistency. The discovery of these two normative dimensions of choice is the main empirical result of this study.

The meaning of these findings can be described quite simply. In most general terms, we have found that peoples' attitudes toward fairness match two normative dimensions: Rawlsian and conservative. Our claim that these are the latent factors of choice is based on the consistency with which choices made over a full range of conflict situations have been observed to fall into the same category. The third, central, category of solutions—the strongly optimal outcomes (Suppes' solutions)—could not have been similarly analyzed given the specifics of our design. In general, however, they do constitute the third component in the distribution of attitudes.

Recall, finally, that the three categories of justice—Rawlsian, conservative, and strongly optimal—have natural political interpretations that correspond to the notions of the left, the right, and the center: Rawlsian solutions are optimal for a group of the worst-off members of the society; the conservative, for a group of the best-off; and the strongly optimal solutions are not biased in either direction and, in this sense, remain central. This connection between the three notions of justice and the three political attitudes opens up yet another set of interesting research problems.

APPENDIX

EXPERIMENTAL PROCEDURES

The Polish Study

The Polish data used in this paper were obtained as a part of a more comprehensive *Values and Choices* study conducted in Warsaw in 1987 and 1988. The larger study dealt with attitudes toward distributive justice, democracy, and social contract (see Lissowski 1993a–b, 1994). One hundred forty-four subjects were recruited among undergraduate students of several universities in Warsaw. The group was selected to include the same number of men and women. Choices that subjects made in the social choice part of the study were independent of payments obtained for participation or payments obtained in other parts of the study.

Instructions Given to Subjects in the First Part of the Social Choice Study (Translation from Polish)

> The jury in a competition consists of three jurors who have to select three candidates, A, B, and C, for three main prizes. Naturally, verdicts of different jurors can be different, perhaps even quite discordant. A number of situations with such discordant verdicts is given below.

Each verdict of a juror is written down as follows: the first letter identifies the candidate, proposed by this juror for the first prize, the second letter, for the second prize, and so on. For example, ABC denotes a verdict to award the first prize to candidate A, the second, to candidate B, and the third, to candidate C.

For each of the situations presented below, determine, taking into account opinions of all three jurors, what you would consider to be the optimal verdict of the jury.

Please note that prizes to three candidates can be awarded in the following 13 different ways:

ABC	BCA	A–BC	AB–C	A–B–C
ACB	CAB	A–CB	BA–C	
BAC	CBA	B–CA	CA–B	

Note that it is possible to make verdicts where equivalent prizes are awarded to two or three candidates. All such verdicts are recorded in the third, fourth and fifth column above. For example, A–BC means that candidates A and B get equal prizes (superior to C's prize), AB–C means that candidates B and C obtain equal prizes (inferior to A's prize), and A–B–C means that all three candidates are awarded equivalent prizes.

Please make your choice of the final jury verdict after a careful analysis of the jurors' opinions. Remember that you act in this experiment as a superior arbiter, who is to determine the final ruling on the basis of individual verdicts of the three jurors. Also remember that you may award equivalent prizes to any two or three candidates; should this be your choice, join the letters denoting candidates to be given equivalent prizes with a dash, as in the examples above.

VERSION 4. SITUATION 1.1

Juror No. 1: CAB
Juror No. 2: ACB
Juror No. 3: BCA
Fill in the best verdict of the jury ☐

Please assess the degree of confidence with which you made your choice of the jury's verdict by underlining one of the following five choices: (1) very uncertain, (2) uncertain, (3) it is difficult to say, (4) certain, (5) very certain.

This instruction continued with an analogous description of Situations 1.2–1.9. Altogether, each subject was presented with nine situations denoted below as profiles I–IX. Four versions of these nine profiles have been used in the study. The design is shown in Table A-1.

The first eight of the nine profiles used in the design represent the eight different types of 3 × 3 profiles. Profile IX belongs to the same type as profile VI: they differ only with respect to the labeling of candidates and order in which jurors' preferences are listed. The four versions are all formally equivalent. Any two versions differ with respect to labeling of candidates (e.g., versions 1 and 3), or order in which jurors' preferences are listed (e.g., 1 and 3), or both order and labeling (e.g., 1 and 4). This design allowed for testing the effects of order and labeling.

The American Study

The American study was much less extensive than the Polish experiment. It was conducted in 1988 on 50 undergraduate students at the State University of New York, Stony Brook. Since this study was done independently from the Polish experiment, the two differed in three aspects: (1) in the American case, students were asked to choose one of six strict orders as a social choice; (2) effects of labeling and order were not controlled for; and (3) in the instruction, jurors were described as voters and competitors as political candidates.

NORMATIVE FOUNDATIONS OF THE KEMENY-SNELL DISTANCE MEASURE

Since there are infinitely many possible distance functions on preference orders, the only way to make some of them applicable to the problem of social choice is by requiring that they have basic normative properties of fair choice procedures. This, in fact, is the only possible source of constraints on a distance function. These normative constraints are formulated as conditions 2, 3, and 4. We begin, however, with an obvious definitional property of a distance function—condition 1.

TABLE A-1

The Experimental Design

	PROFILE								
VERSION	I	II	III	IV	V	VI	VII	VIII	IX
Version 1	ABC BCA CBA	ABC BCA ACB	ABC BCA BAC	ABC ABC BCA	ABC ABC CAB	ABC ABC CBA	ABC ABC ACB	ABC ABC BAC	BCA ACB BCA
Version 2	BCA CAB ACB	BCA CAB BAC	BCA CAB CBA	BCA BCA CAB	BCA BCA ABC	BCA BCA ACB	BCA BCA BAC	BCA BCA CBA	ABC CBA ABC
Version 3	BCA CBA ABC	BCA ACB ABC	BCA BAC ABC	ABC BCA ABC	ABC CAB ABC	ABC CBA ABC	ABC ACB ABC	ABC BAC ABC	BCA BCA ACB
Version 4	CAB ACB BCA	CAB BAC BCA	CAB CBA BCA	BCA CAB BCA	BCA ABC BCA	BCA ACB BCA	BCA BAC BCA	BCA CBA BCA	ABC ABC CBA

It would be fair to say that general properties of a distance function are a part of the conventional understanding of this notion: (1), any distance between two points is understood as a nonnegative number that is zero only if the two points are identical; (2), a distance between two points does not depend on which of the two we measure it from; (3), the direct distance between any two points is always smaller than or equal to any indirect one. If we use p, q, r, ... to represent preference orders and d to denote a distance between them, these three properties of d can be stated as the follows:

Condition 1. *For all* p, q, r, *(i)* $d(p, q) \geq 0$ *and* $d(p, q) = 0$ *if and only if* $p = q$; *(ii)* $d(p, q) = d(q, p)$; *and (iii)* $d(p, q) + d(q, r) \geq d(p, r)$.

The three parts of Condition 1 define what in mathematics is referred to as a metric and thereby reflect and formalize properties of any standard measure of distance.

If a social choice is to be determined as an order that is closest, in some sense, to the preference orders of individual members of the society, the normatively sound distance function ought to agree with individual's ranking of various social choice outcomes. Only then can we reasonably require such fundamental property as Pareto optimality of a social choice outcome. The general problem of what is an individual's preference over social orders (given his own preferences, of course) does not have an easy answer. In fact, it amounts to nothing short of constructing the distance measure itself. However, some special cases turn out to be straightforward. Consider, for example, a domain that consists of two alternatives only. If an individual prefers alternative A to B (AB), then a social choice solution in which A is tied with B (A–B) would, undoubtedly, be closer to his preference, and thus preferred by him as a social outcome, than a solution that places B over A. Geometrically, this would correspond to A–B lying inbetween AB and BA, which would further translate into d(AB, A–B) + d(A–B, BA) = d(AB, BA). Consider now the general case—take any set of alternatives and an order *p* on it. If we ask which one of two orders *q* and *r* is closer to *p*, there is one circumstance in which this general problem has an obvious answer: should we find that for any pair of alternatives, their ordering in *r* is inbetween that in *p* and *q*, then *r* itself is in between *p* and *r*. This is expressed in the next condition.

Condition 2 (Agreement between Individual Preferences and a Distance Function). *The distance measure* d *is such that for all orders* p, q, r, $d(p, r) + d(r, q) = d(p, q)$ *if and only if for any two alternatives* $A, B \in \mathcal{L}$: $p \mid \{A, B\} \succcurlyeq r \mid \{A, B\} \succcurlyeq q \mid \{A, B\}$.

In this formulation, $\mathcal{L}$ stands for any finite set of alternatives, and *p* is a weak order on $\mathcal{L}$. Moreover, for any $\{A, B, \ldots\} \subset \mathcal{L}$, $p \mid \{A, B, \ldots\}$ denotes the *p* order restricted to $\{A, B, \ldots\}$ and relation $\succcurlyeq$ represent the weak preference relation on a pair of alternatives of an individual whose preference order is *p*.

Implications of condition 2 for the optimality of a potential social choice outcome are easy to see. Take, for example, the simplest form of the social choice problem—the case where the society consists of just two individuals. Suppose the preference orders of the two are *p* and *q*. All orders in between *p* and *q* will then be (strongly) Pareto optimal; this will make them—with respect to the optimality criterion—feasible alternatives for the social choice outcome.

The second normative condition, condition 3, follows from an obvious consideration—a requirement that no normatively based choices ought to be affected by the way alternatives are labeled.

Condition 3 (Invariance of Distance with Respect to Relabeling of Alternatives). *Let* $\mathcal{P}: \mathcal{L} \rightarrow \mathcal{L}$ *be any permutation, then for any two orders* p *and* q, $d(p, q) = d(\mathcal{P}(p), \mathcal{P}(q))$.

Finally, the last normative condition specifies changes in the domain of alternatives and the profiles that should have no affect on a measure of distance. Assume, for example, that we have a profile of individual preferences $(p_1, \ldots, p_n)$ on a set of alternatives $\mathcal{L}$ and a social choice order *p*. Suppose now that $\mathcal{L}$ is augmented by one new alternative X, which is such that all individuals prefer, unanimously, X over any alternative in $\mathcal{L}$. (We also assume here that adding X to the set does not change peoples' preferences over other alternatives.) In this case adding X to the domain should not have any influence on the social order of other alternatives. And the same should hold if X were the least preferred alternative by all. Requiring invariance of the distance function with respect to such unanimous and hence inessential changes in the profile is a way to ensure that the social outcome itself will have this normatively desired property.

Condition 4 (Invariance of Distance with Respect to Inessential Changes in the Profile). *The distance function is such that for any two preference orders* p *and* q, *and any new alternative* X, $d(p, q) = d(Xp, Xq) = d(pX, qX)$.

Xp (pX) stands here for an order which puts *X* above (below) all other alternatives in $\mathcal{L}$ and is analogous with *p* on $\mathcal{L}$.

Finally, the last condition specifies a unit of measurement. Since our entire space consists of a finite number of points—and hence a finite set of distances—we can choose, as a matter of convention, to assign the distance of one to the smallest positive distance.

Condition 5 (Unit of Measurement). *The smallest positive distance is 1.*

Now we are ready to formulate the principal conclusion. As it turns out, all that we have essentially done was to provide a normative interpretation of the original Kemeny–Snell conditions. Given that these conditions have already been proved to determine the measure of distance uniquely (Kemeny and Snell 1962, 13–18; see also generalization of Bogart 1973), the proof is particularly easy. The main part of the proof consists of noting that condition 2 implies Kemeny and Snell's axiom 1.3, and condition 4 implies, by induction, their axiom 3. Given, moreover, that our conditions 1, 3, and 5 are the same as their axioms 1, 2, and 4, respectively, the following proposition is a simple corollary of their theorem.

Corollary. *There is only one distance function which satisfies conditions 1–5. The measure is the same as that of Kemeny and Snell.*

In addition to having the desirable normative properties, the Kemeny–Snell measure agrees with some obvious intuitions that one would have about the proximity of certain preference orders. As it seems natural, for example, that ABC is closer to BAC than it is to CBA, and that A–BC is between ABC and BAC, we would expect the measure to reflect it. The Kemeny–Snell distance agrees with all these intuitions. Hence in the Kemeny–Snell measure we get a unique theoretical tool, one that not only complies with our intuitions about proximity of various orders but also satisfies the basic normative conditions that ought to be met by fair social choice procedures.

PROOFS OF PROPOSITIONS 1–3

Proof of Proposition 1. Assume, by contradiction, that there is a kth degree Rawlsian solution $D = (d_1, \ldots, d_n)$ that is not weakly optimal. Such D would have to have larger distances in all coordinates than some other vector, say $D^* = (d_1^*, \ldots, d_n^*)$. This would imply, however, that D is not strongly optimal in any set of coordinates, and hence D^* dominates D in terms of any $>_{Rk}$. But this contradicts the assumption that D is kth degree Rawlsian and proves that all generalized solutions must be weakly Pareto optimal. The reasoning is analogous for a conservative solution.

Proof of Proposition 2. Consider two optimality conditions: strong Pareto optimality and optimality for any one of the $>_{Rk}$ or $>_{Ck}$ relations, for $k = 1, \ldots, n - 1$. Proposition 2 says that neither one of the two optimality conditions implies the other. To prove this proposition we have to construct two models in which only one of the two conditions holds. And so, to prove part i of the proposition, consider the case of profile I in Table 2. In profile I, social orders ABC, BAC, CBA, and A–B–C, are optimal with respect to one of the $>_{Rk}$ and $>_{Ck}$ relations (for $k = 1, 2$), but none of them is strongly Pareto optimal. To prove part ii, consider the following $\Delta = \{1, 2, 4), (0, 1, 5), (2, 2, 3)\}$. Vector (1, 2, 4) is strongly optimal in Δ, since neither (0, 1, 5) nor (2 2, 3) "is better than" (1, 2, 4) in terms of the strong Pareto relation $>$. Yet (1, 2, 4) is neither R1, since (2, 2, 3) has smaller last coordinate, nor R2, since $(2, 3) > (2, 4)$. Finally, a similar reasoning shows that (1, 2, 4) is neither C1 nor C2. This completes the proof.

Proof of Proposition 3. Consider first any two Rk and Rt where $k \neq t$ and $k, t \leq n$. To prove their independence we need to construct a set of distribution vectors with vectors that are Rk and not Rt, Rt and not Rk, both Rk and Rt, and neither Rk nor Rt. First note that since there is always at least one leximin solution and $LR = R1 \cap R2 \cap \ldots \cap Rn$, then for any two levels k and t there always is a solution that is both kth- and tth-degree Rawlsian. Thus what remains to be shown is that there is a domain in which the other three possibilities hold as well. Denote by $d_i(V)$ ith coordinate of vector V and define n ordered vectors of utilities V_k (for $k = 1, \ldots, n$) of length n as follows: $d_i(V_k) = n + k + 1$ for $i = 1, \ldots, k$, and $d_i(V_k) = ni$ for $i = k + 1, \ldots, n$. This definition gives, for example, $V_1 = (n, 2n, 3n, \ldots, n^2)$, $V_2 = (n + 1, n + 1, 3n, \ldots, n^2)$, and, in general, $V_k = (n + k - 1, \ldots, n + k - 1, n(k + 1), \ldots, n^2)$. Note that for any k and t where $k > t$, the last $n - k$ coordinates in V_k and V_t are identical. Now, let's define another set of $n - 1$ vectors V_k^* (for $k = 1, \ldots, n - 1$) as follows: $d_i(V_k^*) = d_i(V_k) - 1$ for $i = k + 1$, and $d_i(V_k^*) = d_i(V_k)$ for $i \neq k + 1$. Finally, take a vector V, say $V = (n - 1, \ldots, n - 1)$, which has all coordinates smaller than the smallest coordinate in all of V_k and V_k^*; V is thus strongly dominated by any one of V_k's and V_k^*'s. Now take a domain consisting of all the vectors constructed so far: $D=\{V_1, \ldots, V_n, V_1^*, \ldots, V_n^*, V\}$. It is a simple exercise to show that any V_k is Rk but is not Rt, for all $t \neq k$. In addition, V is neither Rk nor Rt, for all t and k, which completes the proof of independence of Rk and Rt.

A similar proof can be constructed for any two Ck, Ct; we may actually use the above construction of V_k and V_k^* with utilities transformed by any strictly decreasing function.

Finally, to prove that any Rk and Ct are independent, take the following four vectors of length n: $d_1 = (1, 2, \ldots, n - 1, n)$, $d_2 = (0, 2, \ldots, n - 1, n + 1)$, $d_3 = (1, 2, \ldots, n - 1, n + 1)$, $d_4 = (-1, -1, \ldots, -1, -1)$. Now take domain $D = \{d_1, d_2, d_3, d_4\}$ and note that in this domain (1) d_1 is Rk but not Ct, for all $k \neq t$ ($k, t = 1, \ldots, n$); (2) d_2 is Ct but not Rk, for all $k \neq t$ ($k, t = 1, \ldots, n$); (3) d_3 is Rk and Ct, for all $k \neq t$ ($k, t = 1, \ldots, n$); and (4) d_4 is neither Rk nor Ct, for all k, t ($k, t = 1, \ldots, n$).

Notes

This paper benefited, often indirectly, from comments made by many and on various occasions. With no hope of making our list complete, we would nevertheless like to thank Jonathan Bendor, Michael Cain, Charles Cameron, James Coleman, James Enelow, John Ferejohn, J. Joseph Hewitt, Marek Kaminski, David Lalman, Joe Oppenheimer, the late Klemens Szaniawski, Tadeusz Tyszka, and participants of seminars at the University of Warsaw, University of Chicago, and University of Maryland. Special thanks go to David Johnston, who gave us exceptionally detailed and useful comments on an earlier version of this paper presented at the Public Choice Meetings in New Orleans, 1991.

1. Arrow's Theorem has revealed a problem that continues to be a major dilemma for the theory of democracy and political philosophy (see Riker 1988). The theorem lists a number of normative conditions that a fair rule of aggregating individual preferences should satisfy and concludes that no such rule can be constructed. Conditions like nondictatorship (social outcome is not to be determined by the preferences—strong preferences, to be exact—of a single individual), neutrality (the rule does not favor any of the alternatives), and anonymity (the rule treats all voters equally) are typical of such properties. The power of the Arrow's result comes from the fact that all the required conditions are so fundamental that there can be no good metaprinciple to decide which ones ought to be preserved and which ones can be violated. Formally, however, this is precisely the trade-off that we face when we pick a voting rule: since no rule can meet all conditions on the list, every rule violates some and satisfies others.

2. Research in social choice theory consists mostly of formal results—theorems—that are derived from normative axioms. Such is the case of all the so-called impossibility theorems—refinements on Arrow's theorem (see Kelly 1978). Naturally, for this type of research, any empirical observations are roughly as relevant as it is relevant for the theory of numbers to study those who make arithmetical mistakes.

3. People may make conservative choices believing that they are liberal, or vice versa. Moral principles and political beliefs need not be well defined. But most of all, attitudes often remain latent and can only be inferred from some outcomes. Hence, outcomes may be not only the most reliable index of true attitudes but the only possible one.

4. There is $6^3 = 216$, 3×3 (3 jurors × 3 alternatives) profiles of strict individual preferences. These 216 profiles can be formally partitioned into 10 classes. Two of these classes have, for obvious reasons, been excluded from the study. One class is the set of three identical preference orders, the other is the set of the so-called Condorcet profiles. For a Condorcet profile (e.g., ABC, BCA, CAB) a choice of the single best verdict would be difficult to interpret.

5. A proper definition of the social welfare function involves definitions of other concepts and some degree of formalization. We chose not to introduce this framework, assuming that informal notions would be sufficient to understand the meaning of our results. We should warn, however, that as with any formal construction, well-defined terms are essential and mistakes and misconceptions are easy when

ambiguities are present. For a lucid and careful presentation of all concepts involved, we recommend Sen 1970.

6. Since the concept of the social welfare function is not normatively constrained (although some normative properties, like transitivity, are implicit in the definition), it includes, for instance, a dictatorial rule that designates a single individual and chooses his or her strong preference order as the social order for the whole society. Since rules like that are unlikely to be considered fair, they are unlikely to underlie choices observed in the study. On the other hand, rules like the majority rule, Borda count, and so on, are typically considered as fair or democratic. It would thus make sense to compare their outcomes with choices observed in the study.

7. Majority-related rules include the simple majority rule, Copeland rule, and Dodgson rule. We should note that the simple majority rule violates transitivity on Condorcet profiles. Hence, the rule is not a social welfare function if its domain is unrestricted. In our case, since Condorcet profiles have been excluded, majority rule yields transitive social orders for all profiles used in this study. Positional rules include the standard Borda rule (sum of ranks), as well as its nonstandard versions: restricted, quadratic, and others (see Gärdenfors 1973).

8. Majority rule, for example, is uniquely characterized by the normative conditions of anonymity, neutrality, decisiveness, and positive responsiveness: it is the only rule with these four properties. (This result was proved by May [1952] and is known as May's Theorem. Names of conditions are as in Mueller 1989, 97.) All other rules can, in principle, be described as equivalent to some set of normative properties. Young (1974), for example, established a set of conditions that are equivalent to the Borda count. Yet neither May's nor Young's results are simple: it is not easy to see how and why a specific set of normative conditions uniquely identifies the corresponding rule. Clearly, it would not make much sense to claim that when using rules people consciously realize any of these formal properties. The correspondence between rules and their normative properties is not meant to have any such cognitive denotation.

9. Note that neither CBA in profile I nor ACB in profile II can be explained by any standard social choice function; yet both were frequently chosen (see Figure 2). If we were to speculate on a possible rule which leads to such choices, the following "local majority" rule, which places an alternative in a given position if it is placed in this position by a majority of jurors, does indeed correspond to empirical distribution no. 3. Distribution no. 2 can then be interpreted as a mixture of the simple majority choice for profile I and the "local majority" choice for profile II.

10. Formally, the definition of the Suppes' relation of justice is based on the so-called extended utility functions. The idea of such functions, which was formulated by Arrow (1963, 114–15; see also Sen 1970; Suppes 1977), is that judgments like that are made as a result of an *extended sympathy*—putting oneself in the position of others. This concept is perfectly suited for modeling choices made by subjects in our study. Assumptions 1–3, especially the assumption about full comparability of utilities, allow for constructing extended utility functions of experimental subjects as a function of jurors' utilities for various social choices.

11. Under assumptions 1A, 2, and 3, outcomes of the principles of justice have to be invariant under any linear transformation of utilities, whereas with 1B, 2, and 3, they have to be invariant under any order-preserving transformation of utilities.

12. It is important to remember that parallel coordinates of vectors in Δ need not correspond to the same individual, that is, as we compare any two vectors $D = (d_1, \ldots, d_n)$ and $D^* = (d_1^*, \ldots, d_n^*)$ in Δ, distances d_k and d_k^*, which have the same index in both vectors, would not be typically related to the preference of the same individual.

13. Tomiyama and Sayeki refer to it as Varmin rule.

14. One corollary of proposition 3 will prove to be particularly consequential in the future analyses. Proposition 3 implies that the three general sets of solutions **GR**, **GC**, and **S** are independent; and one effect of independence is that the same social order may be an element of all three sets. The structure of how the three sets overlap will, of course, vary as a function of the domain.

15. This percentage would actually increase from 80.7% to an extraordinary 92.8% if we were to consider as explained these empirical distributions which agree with the generalized solutions in all but one profile. This would be important if we were to use some probabilistic extensions of our model. Another potentially interestingly point to note is that weakly optimal orders—a set larger than that of generalized solutions—explain the same percentage (80.7%) of empirical distributions.

16. If we were to require that each profile in an empirical distribution be explained by the same principle, then the respective percentages would be 18.6% for standard social welfare functions, 27.1% for standard principles of justice, and 41.4% for both.

17. The Polish study had several parts. In one of these parts, subjects were asked to rank (allowing for ties) all strict orders with respect to how close they were to their most preferred choice. For example, someone we chose as the best social order A–BC might have ranked ABC as the closest strict order to his choice, BAC as the second closest, BCA with ACB tied on the third place, and so on. These data allowed for the needed reassignment of social choices with indifferences. One complication here was that these data were not obtained for all subjects in all profiles. Full data were obtained for profile V only; for profiles II–IV, and VI the data were collected from 35 subjects (25% of the group); and in the case of profiles I, VII, and VIII, no data were collected at all. The partial data revealed, however, two strong regularities: (1) two strict orders that were adjacent to the most preferred order with indifference have always been chosen as the closest, and (2) the probability of any one of them being picked as the closest was approximately equal to the proportion of subjects in the entire group who have picked it as the best social choice. We have used these two regularities to generate some of the estimates. Still, the proposed transformation involves a number of arbitrary decisions and generates a hard to estimate error in predictions. The size of this error depends, in part, on the proportion of choices which involved indifferences; these varied from 14–17%, for profiles III, VI, VII, and VIII, to 21% for profile I, to 33% for profiles II and IV, to 43% for profile V.

18. Tomiyama and Sayeki (1982), for example, claim that a large part of the Japanese data is explained by the choices that are egalitarian: BAC in profiles I–IV, ACB in V, ACB and BAC in VI, and ABC in VII and VIII. These are all, however, Rawlsian choices as well—using strict orders as the domain of social choice we cannot separate the egalitarian from the Rawlsian. It is hence possible that what was interpreted as egalitarianism in the Japanese experiment was merely an artifact of using the domain of choice constrained to the strict social choice orders.

References

Arrow, Kenneth J. 1963. *Social Choice and Individual Values.* 2d ed. New York: Wiley.

Black, Duncan. 1958. *The Theory of Committees and Elections.* Cambridge: Cambridge University Press.

Bogart, Kenneth P. 1973. "Preference Structures I: Distances between Transitive Preference Relations." *Journal of Mathematical Sociology* 3:49–67.

Ebbesen, Ebbe B., and Vladimir Konecni. 1980. "On the External Validity of Decision-making Research: What Do We Know about Decisions in the Real World?" In *Cognitive Processes in Choice and Decision Behavior,* ed. T. S. Wallsten. Hillsdale: Earlbaum.

Fishburn, Peter C. 1986. "Empirical Comparisons of Voting Procedures." *Behavioral Science* 31:82–88.

Frohlich, Norman, Joe Oppenheimer, and Cheryl Eavy. 1987.

"Choices of Principles of Distributive Justice in Experimental Groups." *American Journal of Political Science* 31:606–36.

Frohlich, Norman, and Joe Oppenheimer. 1992. *Choosing Justice: An Experimental Approach to Ethical Theory*. Berkeley: University of California Press.

Gärdenfors, Peter. 1973. "Positionalist Voting Functions." *Theory and Decision* 4:1–24.

Kelly, Jerry S. 1978. *Arrow Impossibility Theorems*. New York: Academic.

Kemeny, John G. 1959. "Mathematics without Numbers." *Daedalus* 88:577–91.

Kemeny, John G., and Laurie Snell. 1962. *Mathematical Models in the Social Sciences*. New York: Ginn.

Kern, Lucian. 1978. "Comparative Distributive Ethics." In *Decision Theory and Social Ethics*, ed. Hans Werner Gottinger and Werner Leinfellner. Amsterdam: Reidel.

Lissowski, Grzegorz. 1986. "On the Principles of Distributive Justice." *Oeconomica Polona* 4:473–89.

Lissowski, Grzegorz. 1993a. "Empirical Evaluation of the Probabilistic Principles of Distributive Justice." *Polish Sociological Review* 3(103):249–61.

Lissowski, Grzegorz. 1993b. "Oceny podziałów dóbr: Zasady sprawiedliwości a poczucie sprawiedliwości ludzi" [Evaluations of distributions of goods: Principles of justice versus peoples' perception of justice]. *Studia Socjologiczne* 1(128): 29–50.

Lissowski, Grzegorz. 1994. Ustalanie sposobu podziahu dóbr w sytuacji eksperymentalnej [Determining a Method of Dividing a Good in an Experimental Situation]." *Studia Socjologiczne* 3–4:173–215.

Lissowski, Grzegorz, Tadeusz Tyszka, and Włodzimierz Okrasa. 1991. "Principles of Distributive Justice: Experiments in Poland and America." *Journal of Conflict Resolution* 35:98–119.

May, Kenneth O. 1952. "A Set of Independent Necessary and Sufficient Conditions for Simple Majority Decision." *Econometrica* 20:680–84.

Mueller, Dennis C. 1989. *Public Choice II*. Cambridge: Cambridge University Press.

Rawls, John. 1971. *A Theory of Justice*. Cambridge: Harvard University Press.

Rawls, John. 1974. "Some Reasons for the Maximum Criterion." *American Economic Review* 64:141–46.

Riker, William H. 1988. *Liberalism against Populism*. Prospect Heights, IL: Waveland.

Sen, Amartya K. 1970. *Collective Choice and Social Welfare*. San Francisco: Holden-Day.

Sen, Amartya K. 1974. "Rawls Versus Bentham: An Axiomatic Examination of the Pure Distribution Problem." *Theory and Decision* 4:301–10.

Sołtan, Karol. 1987. *The Causal Theory of Justice*. Berkeley, CA: University of California Press.

Suppes, Patrick. 1966. "Some Formal Models of Grading Principles." *Synthese* 16:284–306.

Suzumura, Kotaro. 1983. *Rational Choice, Collective Decisions, and Social Welfare*. Cambridge: Cambridge University Press.

Tomiyama, Yoshinori, and Yutaka Sayeki. 1982. "Evaluation and Modification of Voting Consequences by Mean-Variance Analysis of Voters' Satisfaction." *Behaviormetrika* 12: 97–109.

Yaari, Menahem E., and Maya Bar-Hillel. 1982. "On Dividing Justly." *Social Choice and Welfare* 1:1–24.

Young, H. Peyton. 1974. "An Axiomatization of Borda's Rule. *Journal of Economic Theory* 9:43–52.

Grzegorz Lissowski is Associate Professor of Sociology, University of Warsaw, Warsaw 00-324, Poland.

Piotr Swistak is Associate Professor of Political Science, University of Maryland, College Park, MD 20742.

RESEARCH NOTES

American Political Science Review Vol. 89, No. 1 March 1995

ECONOMIC AND REFERENDUM VOTING: A COMPARISON OF GUBERNATORIAL AND SENATORIAL ELECTIONS

LONNA RAE ATKESON *University of New Mexico*
RANDALL W. PARTIN *University of Colorado*

We compare vote choice in senate and gubernatorial elections from 1986 and 1990 with two retrospective voting hypotheses: the national referendum hypothesis and an economic retrospective hypothesis. Despite the similarities between the office of U.S. senator and governor (same constituency, high levels of campaign spending, highly visible candidates, etc.), we find that different types of retrospective evaluations are used with respect to vote choice. As members of the national legislative branch, senators' fortunes are linked to the success or failures of the president. In contrast, governors, as state executives, are held accountable for perceived state economic conditions, while senators escape unscathed from the same general economic evaluations. These findings shed some light on the nature of vote choice in a political system complicated by federalism and separation of powers.

Americans live in a complex democracy in which institutions within and across federated levels have separate, yet interdependent, policymaking powers. Interconnections are created not only by constitutional arrangements but by the party system as well. To exercise adequately their role as democratic citizens, Americans need to be able to deal with the problems of assigning responsibility across these levels and institutions. Yet political scientists, understandably fascinated with the way voters exercise their right in the single most important election—the presidential—have yet to unravel the complex nature of vote choice in other, subpresidential elections. In order to uncover some of these possible complexities in vote choice, we apply two alternative theories of voting in subpresidential elections to gubernatorial and senatorial elections. The *national referendum* hypothesis suggests that voters in subpresidential elections express their approval or disapproval of the sitting president and his policies with their vote. In contrast, the *economic voting* hypothesis suggests that voters in these elections express support or dissent for the performance of the incumbent based upon how well the economy is doing.

The central tenet of the referendum theory is that all subpresidential elections are referenda on the performance of the incumbent president and his administration (Simon 1989; Simon, Ostrom, and Marra 1991). If the electorate is satisfied with how things are going and presidential approval is high, candidates of the president's party (incumbents as well as challengers) will benefit from his success. If, on the other hand, domestic or international affairs are going badly, *in-party* candidates will suffer the consequences, losing votes in the November election. Of course, candidates not of the president's party (*out-party* incumbents and challengers) will see their fortunes rise and fall inversely with presidential approval. Evidence supporting a national referendum hypothesis comes mostly from studies of congressional elections at the aggregate level (Abramowitz and Segal 1986; Lewis-Beck and Rice 1984, 1985; Marra and Ostrom 1989; Tufte 1975) and individual level (Kernell 1977; Uslaner and Conway 1984), although there have been a few studies indicating that gubernatorial elections are referenda on presidential policies as well (Piereson 1975; Simon 1989).

The alternative theory of vote choice in these elections comes from the economic retrospective voting literature (see, esp., Fiorina 1981; Key 1966). Simply put, this theory posits a reward–punish decision calculus on the part of the voter. The voter who feels better off financially (or sees the nation as a whole as better off) will reward the incumbent officeholder. Although this instrumental approach to vote choice has gained a great deal of credence in presidential elections (Fiorina 1978, 1981; Kiewiet 1981; Kinder and Kiewiet 1981, 1978; Weatherford 1978; Wildes 1976), its applicability below the level of the national executive is less certain. Aggregate-level studies of the impact of economic conditions on midterm congressional elections have shown mixed success. Many have presented evidence in support of the economic retrospective voting theory (Born 1986; Jacobson and Kernell 1983; Tufte 1975, 1978), while more recent studies have demonstrated no effects of economic conditions on general election outcomes (Alesina and Rosenthal 1989; Erikson 1990). Furthermore, individual-level studies of economic retrospective voting in these elections (Hibbing and Alford 1981; Weatherford 1978) have found a much weaker effect of economic conditions on vote choice than in presidential elections. Finally, the conventional wisdom on state elections (Chubb 1988; Stein 1990) is that insofar as the economy matters, it is not a question of state-level accountability but rather a national-level effect whereby only incumbent candidates of the president's party are rewarded or punished based upon prevailing economic conditions.[1]

These two alternative theories of vote choice in subnational elections (the national referendum and economic voting) have much in common. Both share a retrospective or evaluative explanation of voter behavior: voters ask, "Am I better off today than I was before this administration?" Both also create an accountability model of government. However, the differences between these theories are profound with respect to the two types of elections studied here. Whereas the national referendum hypothesis posits a top–down view of politics and elections by focusing on presidential performance, the state economic voting hypothesis, as supplied here, suggests that state political conditions may influence vote choice in these statewide elections. Finally, the state economic theory focuses specifically on state economic evaluations while the referendum hypothesis posits a more general quality-of-life examination of the president's administration.

THEORETICAL FRAMEWORK AND SUMMARY OF HYPOTHESES

A comparison of senatorial and gubernatorial elections is interesting because of the many similarities between the two offices. Most importantly, senators and governors have the same constituency—a single state. In addition, elections for both offices offer highly visible candidates, strong challengers, and high levels of campaign spending (see Abramowitz 1989; Seroka 1980; Squire and Fastnow 1994; Stein 1990; Wright 1974). Nevertheless, there are also important differences between these two offices, the most obvious pertaining to the branch and level of government. While the senator is a member of the national legislative branch, the governor is the state executive. Because of these differences in level and branch of government, the nature of vote choice in these elections may differ as well. We expect to find that state electorates hold their chief executive, the governor, accountable for state economic conditions to the exclusion of national forces such as presidential approval and evaluations of the national economy. This expectation is in marked contrast to most of the previous research in this area (Chubb 1988; Piereson 1975; Stein 1990; see also Howell and Vanderleeuw 1990). With respect to senate elections, however, we expect to find that vote choice is influenced by presidential approval.

These expectations are consistent with a *federalist* perspective of vote choice in subpresidential elections (see especially Stein 1990). Voters, according to this perspective, recognize and react to the differing responsibilities and circumstances of different branches and levels of government. For example, we would not expect voters to punish or reward local and state elected officials for the health of our national defense or our relationships with other nations. These issues traditionally lie within the realm of the national executive, the president. However, we would expect voters to blame or reward state elected officials for issues such as the condition of the state economy, education, crime, and other policy concerns normally assigned to state and local governments.

Some scholars have argued that cities and states have no real power to protect themselves from labor and capital flight and that state economies are influenced by external, national forces (Elkin 1984; Friedland, Piven, and Alford 1977; Hendrick and Garand 1991; Mollenkopf 1983; Peterson 1981). In this view, state elected officials are simply managers of their respective economies and hence are not politically liable for state economic performance. However, we contend that there is indeed an opportunity for state electorates to reward or punish incumbent officeholders for state economic conditions. Increasingly, states are actively recruiting businesses to their communities through tax incentives, inexpensive land, and cheaper living conditions for their employees. Such actions are taken by state governments in an attempt to improve their economic situation and decrease unemployment levels (Clarke 1986; Grady 1991). In fact, Brace (1991) has shown an increasing influence of state-specific policies in state economic conditions. Voters aware of such actions may evaluate the success or failure of these efforts with respect to the state's economic health and vote accordingly.

State voters may recognize that their state is doing relatively well or relatively poorly in comparison to other states. Alternatively, voters may simply recognize that their state is doing much better or much worse than the nation as a whole. If either is the case, whom do they blame or reward for these differing situations? Is the president responsible for the economies of the 50 states? Are senators, as representatives of their states in the national legislature, responsible? Or does the state government play some role in the state's standing within the national economy? Although the national government may have more room to "fix" the national economy through interest rates and protective tariffs, state officials have some maneuverability to improve or hurt their respective economies.[2]

Consequently, we argue that the responsibility of managing the state economy is laid at the feet of the governor. Governors, like presidents, are executives. The singular nature of their office makes them more vulnerable to public scrutiny and consequently more likely to be held responsible for state issues. In short, by virtue of their position, governors are visible "targets of discontent" (Kirschten 1990). In contrast, it is difficult to blame any single senator for the actions or inactions of the other 99. As a result, while senators may escape from economic retrospective evaluations (particularly at the state level), governors, because of the singular nature of the executive office, may be more susceptible to state economic voting. In a similar vein, the governor, as the leader of the state, is expected to do what is best for the state while senators, as representatives of the states in the national government, may be expected to represent their state's interests balanced against the needs of the entire nation.

If senators are not held directly responsible for the state economy, what are their responsibilities? Recall the assertion that senators may be expected to balance the concerns of their state with issues faced by the entire nation. As a result of these expectations, we argue that a senator's electoral fortunes are tied to the president's perceived success or failure. Because senators are part of a national body and a larger legislative institution, their electoral framework for the voters is national. Such a context inevitably ties them to the activities of the president, who is the singular executive of the entire nation.

Peripheral evidence supports our contention that governors are viewed by voters as responsible for issues concerning the state, while senators are linked to the national government and the president. In particular, Tidmarch, Hyman, and Sorkin (1984) find that news stories about the governor examine state-specific concerns, while senators are often discussed in terms of national issues and their proximity to presidential positions. They also find that governors, whether because of the nature of their office or their location to the local press, are more visible than senators. Finally, research has shown that the governor is the second most recognized elected official, behind the president (Hinckley, Hofstetter, and Kessel 1974; Squire and Fastnow 1994).

Our expectations about vote choice in these elections are that senators, as members of the national legislative branch, will be closely tied to the policies and performance of the sitting president. In short, we expect to find strong evidence of retrospective, national referendum voting in senate races. In contrast, governors, as state executives, will not be affected by prevailing opinions about the performance of the president. Instead, we expect voters to hold their governor accountable for state economic conditions. Furthermore, in both types of races, we argue that accountability will be prevalent only in the reelection efforts of incumbents. The fortunes of incumbent in-party senators rise and fall with presidential approval, while incumbents of the out-party benefit or suffer in the inverse. Incumbent governors, regardless of their partisan ties, are held accountable for worsening or improving state economic conditions. Like Hibbing and Alford (1981), we believe it is unlikely for the electorate to blame or reward open-seat candidates for actions or inactions not directly linked to them. Consequently, open-seat candidates should not be affected by presidential performance evaluations or state economic evaluations.

In addition, a modification to both the economic retrospective voting and the referendum hypotheses is the argument that the retrospective (whether economic or quality-of-life) evaluations are fundamentally linked with the party of the sitting president. Some research on both congressional and gubernatorial elections has concluded that only incumbent candidates of the president's party benefit or suffer from prevailing economic conditions (Hibbing and Alford 1981; Stein 1990). According to this research, out-party incumbents are unaffected by evaluations of economic conditions. The same may hold true for the referendum hypothesis. Only in-party incumbents, because of their partisan and institutional proximity to the president, may benefit or suffer from presidential evaluations. In order to test this incumbency-oriented in-party hypothesis, we shall model vote choice separately based upon the party of the incumbent. Thus we shall be able to observe whether incumbents of the president's party are the only candidates affected by improving state economic conditions (in governor's races) or presidential performance (in Senate races).

DATA AND METHODS

The data used in this analysis come from the 1986 American National Election Study (NES) and its 1990 Senate Election Study.[3] The 1986 study covers 24 states with gubernatorial elections and 23 states with Senate elections, while the 1990 study covers all 36 states with a gubernatorial contest and 31 of the 35 states with Senate elections in the 1990 election year.[4] With the changing contexts, candidates, and different aggregate-level forces present across these two election years, we are presenting a fairly robust test of our hypotheses. Further, 1986 and 1990 are excellent years for us to test our hypotheses. Since 1986 and 1990 are both "mid-term" election years, the races we study here are not being overshadowed by the high profile presidential election. Indeed, most gubernatorial contests are now held in these "off-years" for that very reason (Tompkins 1988). In addition, a wide range of economic conditions are prevalent in 1986 and 1990. In 1986, the mean change in real disposable income per capita from 1985 to 1986 was a positive 4.62%, with the least growth found in North Dakota (.84%) and the most in New York (9.93%). Economically, with even more across-state variation than in 1986, in 1990, 49 of the 50 states had positive increases in their disposable income per capita from the previous year, with the mean increase an impressive 7.19%. (Nevada saw a 2.03% drop in its disposable income per capita while South Dakota experienced a 15.95% increase in the same economic measure.) State unemployment in 1990 was also relatively low, as indicated by the mean unemployment rate of 5.2%.[5] Thus, these two election years provide a stiff test of our hypotheses, as economic times were good, with a fair amount of variation across the states, making subtle differences between the states all the more important if state economic factors do play a role in state elections.

The 1986 NES survey offers a preliminary, suggestive test of our hypotheses. In particular, we must be concerned with the national sampling frame of this survey. As a result, there will be many more respondents from more populous states than from less populated states. Because our interest is in a cross-sectional comparison of vote choice in all states, any national sample of voters is suboptimal: here we are as interested in the vote choice made in Wyoming as that made in New York (Westlye 1983). However, in spite of this limitation, if we find even modest support for our hypotheses among the 1986 sample, we contend that we may still say something about the nature of vote choice in a political system complicated by federalism and separation of powers. Fortunately, the 1990 Senate Election Study does not suffer from this same limitation, because its sampling frame emphasized across-state parity concerns over national sampling concerns. Thus, the 1990 Senate Election Study offers us the best opportunity to test our hypotheses.

The dependent variable used here is the vote of the respondent. The variable is dichotomous and represents a choice of the Democratic (0) or Republican (1) candidate for the office in question (either senator or governor).[6] The dichotomous nature of our dependent variable precludes the use of ordinary least squares, since the assumption of linearity would be violated. As a result, the nonlinear logistic regression method of estimation will be used.

We apply identical models to both senate and gubernatorial races. In both races we expect to find evidence supporting a retrospective theory of voting. However, each race should differ with respect to where that retrospective judgment will be found. By applying identical models to both types of races, we are allowing the national referendum model and state economic voting model to compete against one another. We include individual presidential approval scores in our model to test the notion that subnational elections are "referenda" on the president and his policies. This presidential approval variable is coded 1 through 5, with a value of 1 representing *strong disapproval*, and a value of 5, *strong approval*, of the president's performance.

Our model also includes the respondent's evaluation of the contemporary economic situation to test the economic retrospective voting hypothesis. Respondents were asked to evaluate their personal financial situation, the economic condition of their state, and the economic condition of the nation as a whole. These variables are coded 1 through 5, with a value of 1 representing *much worse*, and a value of 5, *much better*. (A value of 3 indicates an *about the same* response). Because we have information on economic perceptions at three separate levels, we shall include them all. Thus if an assignment of economic responsibility is at all present, we will be able to detect at which level that accountability rests (at the national, state, or "personal pocketbook" level). We include personal economic conditions because some scholars have concluded that the individual's economic condition should be the decisive factor (Campbell, et al. 1960), while others believe that the nation's economic health (as perceived by the voter) is the decisive factor (Kinder and Kiewiet 1981). We expect, however, that insofar as there is economic voting in these elections, it will be a the level of the state: perceptions of the state economy will be the predominant focus of our inquiry.

Finally, variables that will be included as "controls" are *partisanship* and *ideology* of the respondent. Political scientists have long known that such variables play an important role in influencing voting behavior (Campbell et al. 1960). They are needed as controls in our models to separate partisan differences and perceptions from economic and presidential performance evaluations. Both of these variables are based upon responses to a survey question and are represented by seven-point scales. For the party identification variable, a value of 1 represents a strong Republican affiliation and a value of 7 represents a strong Democratic affiliation. For the ideology variable, a value of 1 indicates an extremely conservative self-placement, and a value of 7 indicates an extremely liberal self-placement.

Formally, our model takes the following shape:

$$Y = a + b_1(\mathrm{PE}) + b_2(\mathrm{SE}) + b_3(\mathrm{NE}) + b_4(\mathrm{PA}) + b_5(\mathrm{PTY}) + b_6(\mathrm{ID}) + e,$$

where Y = vote choice, PE = personal economic evaluations, SE = state economic evaluations, NE = national economic evaluations, PA = presidential approval, PTY = strength of party identification, ID = ideological strength on the liberal–conservative scale, and e = error term.

RESULTS

The results from an initial test of the two hypotheses are presented in Tables 1 and 2.[7] Table 1 presents the logistic regression coefficients for the contested Senate races held in 1986 and 1990, while Table 2 presents the results from an analysis of the gubernatorial races held those same years. Results from races where there was a Republican incumbent (the same party as the president in 1986 and 1990) are presented next to those for races where the incumbent was a Democrat (the out party in both 1986 and 1990) and those for races where the incumbent retired to leave an open seat. For the Senate races, we expect to find a strong and significant relationship between presidential approval and vote choice, with no evidence of economic retrospective voting. In contrast, we expect to find economic voting in gubernatorial elections and little or no national referendum voting. Furthermore, if the aforementioned incumbency-oriented in-party hypothesis were correct, we would find state economic voting (for governors) and referendum voting (for senators) only in races with a Republican incumbent.

The results from the Senate races suggest a rejection of the economic retrospective voting hypothesis as expected: none of the individual-level economic evaluations (at the personal, state, or national levels)

TABLE 1

Logistic Regression Model of Vote Choice for the Republican Candidate in Senate Races, 1986 and 1990

INDEPENDENT VARIABLES	DEMOCRATIC INCUMBENT SENATOR		REPUBLICAN INCUMBENT SENATOR		OPEN RACE	
	1986	1990	1986	1990	1986	1990
Personal economic conditions	−.038 (.225)	.054 (.113)	−.099 (.153)	.088 (.121)	−.258 (.474)	.235 (.288)
State economic evaluations	.317 (.238)	−.119 (.120)	−.137 (.162)	−.031 (.122)	.402 (.452)	−.357 (.321)
National economic evaluations	−.312 (.256)	−.290 (.154)	.090 (.167)	.099 (.167)	.142 (.492)	.216 (.424)
Presidential approval	.591** (.190)	.266** (.093)	.168 (.100)	.328*** (.083)	1.981* (.848)	.134 (.218)
Party Identification	−.508*** (.115)	−.539*** (.068)	−.525*** (.080)	−.377*** (.069)	−.380 (.257)	−.934*** (.212)
Ideology	−.258 (.206)	−.166* (.079)	−.234* (.118)	−.165* (.077)	.147 (.452)	−.466* (.193)
Constant	.417 (1.443)	1.424* (.677)	3.294*** (.880)	1.881** (.703)	−7.623 (4.261)	5.417** (1.922)
N	196	536	303	477	58	122
−2 log likelihood	151.958	498.340	311.280	425.639	31.332	77.819
% correctly predicted	82.65	76.49	76.57	79.66	84.48	86.89

Note: For economic evaluations, a high number indicates better economic conditions (1–5 scale); for presidential approval, a high score represents stronger support for the president (1–5 scale); the partisanship variable is coded so that strong Republicans are 1 and strong Democrats are 7 (1–7 scale); ideology is coded so that a high score indicates a more liberal position (1–7 scale). Standard errors are in parentheses.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

prove to have a significant effect (at $p < .05$) on vote choice in these elections regardless of the type of race, although the effect of national economic evaluations in races with a Democratic incumbent in 1990 is quite close to being statistically significant. With respect to the presidential approval variable, however, the results are quite different. In races with an incumbent of either party, the coefficient for presidential approval is both strong and significant, with the obvious anomaly of races with a Republican incumbent senator in 1986. This supports the argument that there is an element of a presidential "referendum" at work in these Senate elections. Republican candidates (incumbents as well as challengers) benefited from a positive evaluation of the sitting president in 1990, while Democratic incumbents in both 1986 and 1990 and Democratic challengers in 1986 benefited from a negative evaluation of the sitting president. In short, voters in Senate races express their support for or dissatisfaction with the president by respectively rewarding or punishing candidates of his party—a national referendum effect. In addition, party identification is significant and in the expected direction. The more Republican a voter, the more likely he or she supported the Republican candidate. Finally, in all of the Senate races from 1990 and in races with an incumbent from the GOP in 1986, liberalism was significantly and negatively related to a Republican vote.

This support for the referendum voting hypothesis in Senate elections fits well with our federal interpretation of Senate elections. The fortunes of incumbent senators, as members of the national legislature, are tied to presidential performance. However, in open races, the national referendum effect was present only in 1986 and not found at all in 1990; and the results from the 1986 open race model are questionable due to the small number of respondents. In the 1990 open-race model, party identification and ideology are the only significant variables, as we would expect. Because candidates in open races do not have a record related to the president's policies, it would seem implausible for voters to evaluate them in terms of presidential performance. This indicates that voters were not blaming or rewarding open-seat candidates based upon their evaluations of the president and his policies.

The results from a test of the same hypotheses on gubernatorial elections (presented in Table 2) offer very different conclusions. To begin, whereas presidential approval was shown to be strongly affecting vote choice in senatorial elections, in gubernatorial elections the effect is not consistently present. Only in 1986 races either with a Democratic incumbent or in open races does the presidential approval measure meet the level of statistical significance. Certainly, this evidence (admittedly, from our weakest data) does not substantiate the referendum hypothesis,

TABLE 2

Logistic Regression Model of Vote Choice for the Republican Candidate in Gubernatorial Races, 1986 and 1990

INDEPENDENT VARIABLES	DEMOCRATIC INCUMBENT GOVERNOR		REPUBLICAN INCUMBENT GOVERNOR		OPEN RACE	
	1986	1990	1986	1990	1986	1990
Personal economic conditions	−.183 (.181)	.005 (.123)	.196 (.248)	−.247 (.126)	−.095 (.176)	.200 (.120)
State economic evaluations	−.391** (.149)	−.276* (.139)	.588* (.261)	.651*** (.151)	.167 (.176)	−.182 (.125)
National economic evaluations	.127 (.176)	−.092 (.158)	−.089 (.269)	.064 (.181)	.010 (.196)	−.188 (.170)
Presidential approval	.545*** (.139)	.034 (.096)	.291 (.181)	.123 (.093)	.330** (.123)	.150 (.084)
Party identification	−.425*** (.089)	−.435*** (.072)	−.544*** (.137)	−.481*** (.071)	−.490*** (.089)	−.559*** (.071)
Ideology	−.099 (.141)	−.100 (.083)	−.731*** (.211)	−.140 (.080)	−.114 (.140)	−.109 (.078)
Constant	.645 (1.008)	2.117** (.755)	2.698 (1.542)	1.233 (.764)	.669 (.906)	2.402*** (.675)
N	296	380	167	378	225	413
−2 log likelihood	265.539	417.814	114.726	390.912	229.757	437.371
% correctly predicted	78.04	72.89	82.04	75.66	74.22	75.79

Note: For economic evaluations, a high number indicates better economic conditions (1–5 scale); for presidential approval, a high score represents stronger support for the president (1–5 scale); the partisanship variable is coded so that strong Republicans are 1 and strong Democrats are 7 (1–7 scale); ideology is coded so that a high score indicates a more liberal position (1–7 scale). Standard errors are in parnetheses.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

which suggests that *all* subpresidential elections are used by the voters as national referenda on the president's policies. However, it does suggest that there may occasionally be such an effect. In every other type of gubernatorial race studied here (races in 1986 and 1990 with a Republican incumbent and races with a Democratic incumbent in 1990), the presidential approval measure does not significantly affect vote choice.

More importantly, in contrast to the lack of economic voting found in Senate elections, we do find state economic retrospective voting in gubernatorial elections. In 1986 and 1990, perceptions of state economic conditions affect vote choice in gubernatorial elections with an incumbent of either party. The negative sign in races with a Democratic incumbent shows that a perceived improvement in the state economy hampers the success of the Republican candidate (the challenger in this case)—a reward for the Democratic incumbent. Similarly, the positive coefficients in races with a Republican incumbent indicate that a perceived improvement in the state economy helps Republican incumbents and hurts Democratic challengers. Although the effect is visibly stronger in races with a Republican incumbent, the fact that it is also felt in races with a Democratic incumbent lends credence to the simple state economic retrospective voting hypothesis: incumbent governors are held accountable for state economic conditions. Not surprisingly, then, the effect does not exist in races where there is no incumbent. Again, candidates for an open gubernatorial seat, like their senatorial counterparts, are not held responsible for past events. Finally, in all types of gubernatorial elections in both 1986 and 1990, partisanship significantly affected vote choice.

Because Republican incumbent governors consistently seem to be more affected by perceived state economic conditions (as demonstrated by the larger coefficients), there may exist some sort of in-party accentuation of economic voting in those races. Although previous research would lead us to believe that this is the case (Chubb 1988; Stein 1990), we can only speculate that these governors were punished more severely or rewarded more handsomely because of their shared partisan ties to the sitting president. After all, for both of the years studied here—1986 and 1990—a Republican occupied the White House. Nevertheless we find this result intriguing and believe a more thorough examination of its implications may be useful. In Table 3, we demonstrate the impact of differing economic evaluations on vote choice in elections with a gubernatorial

TABLE 3

Predicted Percent Probability Voting Republican in Governor's Races, 1986 and 1990

EVALUATIONS OF THE STATE ECONOMY	DEMOCRATIC INCUMBENT 1986	DEMOCRATIC INCUMBENT 1990	REPUBLICAN INCUMBENT 1986	REPUBLICAN INCUMBENT 1990
Much worse	42.85	41.00	41.39	33.20
Worse	33.65	34.53	55.98	48.80
Stayed the same	25.54	28.58	69.59	64.62
Better	18.83	23.28	80.46	77.80
Much better	13.56	18.73	88.12	87.05

Note: The percent probability predictions presented here come from the log–odds ratio derived from multiplying all of the independent variables in our model (see Table 2) by their respective mean values, excluding the state economic conditions measure. The product of the state economic evaluations coefficient and the possible values for that variable (1–5) are then added. For each value of the independent variable, a different probability prediction is then calculated. This table is illustrative of the relative effect of state economic evaluations on vote choice for this particular model. For an extended discussion of the interpretation of logistic regression coefficients, see Hosmer and Lemeshow, 1989.

incumbent by using probability predictions derived from the results presented in Table 2.

Setting all of the variables (except state economic evaluations) to their mean values, we calculate a percent probability prediction of a Republican vote in these races based upon differing values of the state economic condition variable. For example, a person living in a state with a Republican incumbent governor in 1990, with mean responses of all of the other variables included in the model and who perceives the state economy as having become "much worse" over the past year will vote for the incumbent with a probability of about 33%. However, another voter, with mean responses to all of the other variables and who perceives the state economy as having become "much better" over the past year, will support the Republican incumbent in this race with a percent probability of about 87%. The difference, then, is slightly less than a 54% shift in probability of voting for the Republican incumbent based upon differences in perceptions of the state economy. This exhibits a strong effect of state economic conditions in races with a Republican incumbent governor.

In contrast, although there is a significant effect of state economic conditions on vote choice in races with an incumbent Democratic governor, it is not nearly as strong as it is for Republican incumbents. For example, the extremes for voters in states with Democratic incumbent governors in 1990 are a 41% probability of voting for the Republican challenger (state economy is "much worse") and about a 19% probability prediction of voting for the Republican challenger ("state economy much better"). Indeed, the differences in probability of a Republican vote between the two extreme responses to the state economic conditions question (*much worse* versus *much better*) are much lower for voters in states with Democratic incumbents (29% in 1986, 22% in 1990) than for voters in states with Republican incumbents (47% in 1986, 54% in 1990).

In these races, Democratic and Republican governors were rewarded for a perceived improvement in the state economy. However, GOP governors were even more susceptible to poor evaluations of state economic conditions than Democratic governors, and Republican challengers were not helped by a perceived weakening of the state economy the way Democratic challengers were. If future research uncovers similar results when a Democrat occupies the White House, then an altogether new modification of the incumbency-oriented in-party hypothesis will be in order. Recall that Stein (1990) found that only incumbent governors of the president's party benefit or suffer from economic evaluations, thus suggesting little, if any, state-level accountability. Our results indicate that while incumbents from both parties were held responsible for state economic conditions and incumbents of all stripes were almost equally rewarded for improving state economic conditions, in-party incumbents were more affected by a perceived worsening of the state economy than their out-party counterparts.

CONCLUSION

The comparison of gubernatorial and senatorial vote choice in two different election contexts supports our initial hypotheses and provides us with some interesting insights to the characteristics of these elections. We find a strong national referendum effect in Senate races whereby candidates of the president's party (challengers as well as incumbents) benefit or suffer from the popularity of the sitting president. Furthermore, in senatorial elections, we do not find any significant effect of economic evaluations on vote choice. In marked contrast, we find that governors, as chief executives of their respective states, are held responsible for the health of their state economies and are not generally shown to be liable for fluctuations in presidential approval.

These findings offer a complex picture of vote choice. Our evidence of referendum voting in Senate elections supports the findings of Simon, Ostrom, and Marra (1991), while the lack of a comparable effect in gubernatorial elections contradicts their assertion that all subnational elections are referenda on the sitting president. Similarly, our evidence of state economic retrospective voting in gubernatorial elections contradicts most of the conventional wisdom on these elections (Chubb 1988; Stein 1990), although we do find something of an in-party incumbency effect whereby incumbent governors of the president's party suffer more from a perceived worsening of state economic conditions than incumbents of the out party—a finding not too different from Stein's (1990).

Given that both senators and governors have the same constituency—the state voters—the differences uncovered here support a federal interpretation of the two offices. Senators, as members of the national legislative branch, are linked by voters to the national government and are hence susceptible to evaluations of the president. In contrast, governors, as state executives, escape from these national-level evaluations of presidential performance and are instead held liable for state economic conditions. This suggests that despite their similar constituencies, governors and senators are held responsible for different agendas. The federal nature of our government places a different emphasis and perspective on each office. As a result, the electorate holds candidates "functionally responsible" for the agenda assigned to their respective offices. This conclusion is consistent with other research in this area, which has found that voters are able to differentiate the responsibilities of different political offices and behave according to those assigned responsibilities when they vote (Stein 1990).

Notes

We wish to thank Walter J. Stone for his helpful comments, suggestions, and criticisms on earlier drafts of this paper. The names of the authors appear in alphabetical order: this paper is in every way a collaborative enterprise. An earlier version was presented at the annual meetings of the Western Political Science Association, Pasadena, 1993. The data utilized in this paper were made available by the Inter-University Consortium for Political and Social Research.

1. Stein (1990) argues that this finding supports a "functional responsibility" interpretation of federal elections. Governors are not "functionally responsible" for state economic conditions, and incumbent governors are thus not held accountable by state electorates; incumbents of the president's party are used by state electorates to send a message to the president about the economic condition in the states and regions.

2. Furthermore, as pointed out by Peffley (1985), responsibility for economic conditions may be assigned even though elected officials may not have the power or authority to affect change; accountability is a function of the position held, rather than the activities of the person holding the position.

3. Unfortunately, prior to 1986, the NES did not ask questions pertaining to state economic conditions, which is necessary if we are to test for state-level economic voting. In addition, the 1988–92 NES Senate Election Study did not ask about gubernatorial vote choice during the presidential election years of 1988 and 1992. In short, the 1986 NES study and the 1990 Senate Election Study offer us the two most readily accessible data with which to test our hypotheses. Elsewhere we have focused solely on senate elections, using the 1988 and 1992 segments of the Senate Election Study. Because we are unable to compare these results with those from our analyses of gubernatorial vote choice, we do not present them here.

4. Four of the Senate races in 1990 were uncontested. In two states, the Democratic senator ran unopposed (Arkansas and Georgia) and in two others, the Republican ran unopposed (Mississippi and Virginia). Because our hypotheses deal with the necessity of casting a dissenting vote for the incumbent, we exclude those races where there was no challenger.

5. The state unemployment data come from the NES Senate 1990 study used in our analysis. The percent change in real disposable income per capita for the states is calculated from *Sales and Marketing Management*'s annual survey of buying power.

6. In two of the gubernatorial races (Alaska and Connecticut), there were highly visible Independent candidates. For present purposes, we have excluded the votes cast for any Independent candidates in any race. Indeed, in the entire 1990 NES Senate Study sample, only 17 respondents supported an Independent candidate.

7. Before we tested the two alternative hypotheses, we examined the bivariate relationships between the various economic indicators because we were concerned with multicollinearity. However, we found little evidence to support a concern. The lack of collinearity is consistent with the findings of Kiewiet (1983, 88–95).

References

Abramowitz, Alan I. 1989. "Campaign Spending in the U.S. Senate Elections." *Legislative Studies Quarterly* 14:487.

Abramowitz, Alan I., and Jeffrey Segal. 1986. "Determinants of the Outcomes of Senate Elections." *Journal of Politics* 48:433–40.

Alesina, Alberto, and Howard Rosenthal. 1989. "Partisan Cycles in Congressional Elections and the Macroeconomy." *American Political Science Review* 83:373–98.

Born, Richard. 1986. "Strategic Politicians and Unresponsive Voters." *American Political Science Review* 80:599–612.

Brace, Paul. 1991. "The Changing Context of State Political Economy." *Journal of Politics* 53:297–317.

Campbell, Angus E., Philip E. Converse, Warren E. Miller, and Donald E. Stokes. 1960. *The American Voter.* New York: Wiley.

Chubb, John E. 1988. "Institutions, the Economy, and the Dynamics of State Elections." *American Political Science Review* 82:133–54.

Clarke, M. K. 1986. *Revitalizing State Economies: A Review of State Economic Development Policies and Programs.* Washington: National Governor's Association Press.

Elkin, Stephen. 1984. "Twentieth Century Urban Regimes." *Journal of Urban Affairs* 8:11–28.

Erikson, Robert S. 1990. "Economic Conditions and the Congressional Vote: A Review of the Macrolevel Evidence." *American Journal of Political Science* 34:373–99.

Fiorina, Morris P. 1978. "Economic Retrospective Voting in American Elections: Micro-analysis." *American Journal of Political Science* 27:426–43.

Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections.* New Haven: Yale University Press.

Friedland, Roger, Frances Fox Piven, and Robert Alford. 1977. "Political Conflict, Urban Structure and the Fiscal Crisis." In *Campaigning for Public Policy,* ed. Douglas Ashford. Beverly Hills: Sage.

Grady, Dennis. 1991. "Managing the State Economy: The Governor's Role in Policymaking." In *Gubernatorial Leadership and State Policy,* ed. Eric B. Herzik and Brent W. Brown. Westport: Greenwood.

Hendrick, Rebecca M., and James C. Garand. 1991. "Variation in State Economic Growth: Decomposing State, Regional, and National Effects." *Journal of Politics* 53:1091–1110.

Hibbing, John R., and John R. Alford. 1981. "The Electoral Impact of Economic Conditions: Who Is Held Responsible?" *American Journal of Political Science* 25:423–39.

Hinckley, Barbara, Richard Hofstetter, and John Kessel. 1974. "Information and the Vote: A Comparative Election Study." *American Politics Quarterly* 2:131–58.

Hosmer, David W., Jr., and Stanley Lemeshow. 1989. *Applied Logistic Regression.* New York: John Wiley & Sons.

Howell, Susan E., and James M. Vanderleeuw. 1990. "Economic Effects on State Governors." *American Politics Quarterly* 15:471–83.

Jacobson, Gary C., and Samuel Kernell. 1983. *Strategy and Choice in Congressional Elections.* New Haven: Yale University Press.

Kernell, Samuel. 1977. "Presidential Popularity and Negative Voting." *American Political Science Review* 71:44–66.

Key, V. O. 1966. *The Responsible Electorate.* New York: Vintage.

Kiewiet, D. Roderick. 1981. "Policy Oriented Voting in Response to Economic Issues." *American Political Science Review* 75:448–59.

Kiewiet, D. Roderick. 1983. *Macroeconomics and Micropolitics: The Electoral Effects of Economic Issues.* Chicago: University of Chicago Press.

Kinder, Donald R., and D. Roderick Kiewiet. 1978. "Economic Discontent and Political Behavior: The Role of Personal Grievances and Collective Economic Judgements in Congressional Voting." *American Journal of Political Science* 23: 495–527.

Kinder, Donald R., and D. Roderick Kiewiet. 1981. "Sociotropic Politics: The American Case." *British Journal of Political Science* 11:125–61.

Kirschten, Dick. 1990. "Targets of Discontent." *National Journal,* 10 November 1990, pp. 2736–42.

Lewis-Beck, Michael, and Thomas Rice. 1984. "Forecasting U.S. House Elections." *Legislative Studies Quarterly* 9:475–86.

Lewis-Beck, Michael, and Thomas Rice. 1985. "Are Senate Election Outcomes Predictable?" *PS* 18:745–55.

Marra, Robin F., and Charles W. Ostrom, Jr. 1989. "Explaining Seat Change in the U.S. House of Representatives, 1950–1986." *American Journal of Political Science* 33:541–69.

Mollenkopf, John. 1983. *The Contested City.* Princeton: Princeton University Press.

Peffley, Mark. 1985. "The Voter as Juror: Attributing Responsibility for Economic Conditions." In *Economic Conditions and Electoral Outcomes,* ed. Heinz Eulau and Michael S. Lewis-Beck. New York: Agathon.

Peterson, Paul. 1981. *City Limits.* Chicago: Chicago University Press.

Piereson, James E. 1975. "Presidential Popularity and Midterm Voting at Different Electoral Levels." *American Journal of Political Science* 19:683–94.

Seroka, Jim. 1980. "Incumbency and Reelection: Governors Versus U.S. Senators." *State Government* 52:161–65.

Simon, Dennis M. 1989. "Presidents, Governors, and Electoral Accountability." *Journal of Politics* 51:286–304.

Simon, Dennis M., Charles W. Ostrom, Jr., and Robin F. Marra. 1991. "The President, Referendum Voting, and Subnational Elections in the United States." *American Political Science Review* 85:1177–92.

Squire, Peverill, and Christina Fastnow. 1994. "Comparing Gubernatorial and Senatorial Elections." *Political Research Quarterly* 47:705–720.

Stein, Robert M. 1990. "Economic Voting for Governor and U.S. Senator: The Electoral Consequences of Federalism." *Journal of Politics* 52:29–53.

Tidmarch, Charles M., Lisa J. Hyman, Jill E. Sorkin. 1984. "Press Issue Agendas in the 1982 Congressional and Gubernatorial Election Campaigns." *Journal of Politics* 46:1226–42.

Tompkins, Mark E. 1988. "Have Gubernatorial Elections Become More Distinctive Contests?" *Journal of Politics* 50: 192–205.

Tufte, Edward R. 1975. "Determinants of the Outcomes of Midterm Congressional Elections." *American Political Science Review* 69:812–26.

Tufte, Edward R. 1978. *Political Control of the Economy.* Princeton: Princeton University Press.

Uslaner, Eric M., and M. Margaret Conway. 1985. "The Responsive Congressional Electorate: Watergate, the Economy, and Vote Choice in 1974." *American Political Science Review* 79:788–803.

Weatherford, M. Stephen. 1978. "Economic Conditions and Electoral Outcomes: Class Differences in the Political Response to Recession." *American Journal of Political Science* 22:917–38.

Westlye, Mark C. 1983. "Competitiveness of Senate Seats and Voting Behavior in Senate Elections." *American Journal of Political Science* 27:253–83.

Wildes, Jeffrey W. 1976. "Self-perceived Change and Political Orientations: A Preliminary Exploration." *American Politics Quarterly* 4:395–412.

Wright, Gerald C., Jr. 1974. *Electoral Choice in America.* Chapel Hill, NC: Institute for Research in Social Science.

Lonna Rae Atkeson is Assistant Professor of Political Science at the University of New Mexico, Albuquerque, NM 87131.

Randall W. Partin is a Doctoral Candidate at the University of Colorado, Boulder, CO 80309-0333.

DISENTANGLING PATTERNS OF STATE DEBT FINANCING

JAMES C. CLINGERMAYER *University of Texas, El Paso*
B. DAN WOOD *Texas A&M University*

We examine the determinants of change in state government indebtedness from 1961 through 1989 using a pooled time series cross-sectional analysis. The analysis reveals that debt is primarily a function of economic conditions reflecting both the need to borrow and the capacity of states to repay debt. However, political factors such as culture, partisan competition, and electoral cycles also affect state debt. We also find very weak evidence that tax and expenditure limitations, ironically, may increase state indebtedness, while constitutional debt limitations have no effect upon slowing the growth of state debt.

For much of our history, Americans believed that the appropriate norm of government fiscal conduct was a balanced budget. Borrowing was to be avoided; debt was seen as a threat to the solvency of governments and their citizens. This was a norm that was frequently violated, with governments borrowing to build canals and turnpikes, as well as other roads, and to finance wars. Nevertheless, the norm of balanced budgets was widely held by the public. Occasional defaults by state and local governments served to strengthen that sentiment and to motivate support for state constitutional restrictions upon debt financing during the twentieth century.

Today, deficit financing by the federal government has become expected. State and local government borrowing has also grown, but it is widely believed that debt limitations, separated capital budgets, and norms of state budget balance have limited the growth in state debt. Most of the growth in subnational debt since the 1960s has been in the form of nonguaranteed debt (Regens and Lauth 1992). State governments are often thought to be more fiscally responsible than the national government, immune to the same sort of tendencies characterizing the federal level. In fact, partisans at the national level, troubled by the growing national debt and rising annual deficits, have called for constitutional balanced budget amendments and new debt limitations similar to those found at the state level. Additionally, proposals for limitations upon taxing and spending, similar to those already in place at the state level, have also been introduced.

We shall examine the causes of public debt, using states as a quasi-experimental laboratory. The number of cases and institutional and demographic variation available to state-level analysis provide a more general framework for testing explanations of public debt. Many of the specific proposals for controlling government borrowing at the federal level have already been employed by the states, including constitutional debt limitations and taxing and spending limits. Research on state debt is also important because of the impact that debt service may have upon the service capacity and fiscal integrity of the governments that provide many of the most important programs affecting U.S. citizens. In recent years we have seen fiscal crises develop in such states as California, New York, and Massachusetts, which had high debt-servicing costs during times of economic decline and revenue shortfalls. Yet there is very little research to guide our understanding of why such fiscal hardships develop.

PAST EXPLANATIONS FOR PUBLIC DEBT

For centuries governments have borrowed money for a variety of reasons: to finance long-lived infrastructure, to cover temporary shortfalls in revenue, to bankroll wars, and (more recently) to counteract cyclical downturns in economic activity. Yet for much of American history, federal budget deficits and the resulting national debt were not considered much of an issue, because deficits were not particularly common. In fact, for most of the nineteenth century, the federal government experienced annual budget surpluses far more frequently than it suffered deficits (Anderson 1987). State governments, on the other hand, were frequently in debt during the nineteenth century and also fairly often lapsed into default. The states' reputation for fiscal irresponsibility prompted the passage of constitutional debt limitations during the late nineteenth and early twentieth centuries.

Throughout U.S. history the most common purpose of debt financing has been the provision of long-lasting infrastructure. Bridges, dams, roads, railway lines, sewer systems, and fresh water supplies have all been financed through borrowing. This has been justified by the so-called "pay-as-you-use" principle, which posits that it is desirable for individuals who enjoy a facility or service to pay for its provision. If infrastructure were paid for by current revenues, some generations would enjoy considerable benefits from facilities financed in the past but would pay little for their creation. Thus, debt financing of capital projects has often been justified as distributing costs across all users of capital improvements, rather than just across current users. Such borrowing can also be justified as a means of reducing volatility in tax rates (Barro 1979).

With fairly great frequency, state and local governments also engage in short-term borrowing to cover temporary operating deficits that occur when expected revenues do not match regular, scheduled outlays. In these cases, tax collections may be behind schedule, intergovernmental aid may not be allotted on time, or states may simply take advantage of favorable money markets. Some of these short-term issues are later refinanced with long-term bonds. These purposes for short-term borrowing are widely accepted, although excessive short-term debt may produce the impression of long-run economic problems.

In contrast to the federal government, state and local governments often maintain two separate budgets, one for regular annual operating expenses and the other for capital projects. However, the distinction between operating and capital expenditures has become blurred through the years, with many claiming that "bricks-and-mortar" projects are not the only projects belonging in a capital budget and, therefore, eligible for debt financing. Nevertheless, some public finance experts insist that capital budgets make state and local government fundamentally different from a federal government that has no separate capital budget.

During the twentieth century debt financing by the national government has become common as a way of funding expenditures to stimulate demand during periods of high unemployment and underutilized capital. This practice, commonly associated with the writings of John Maynard Keynes, is not widely recommended for subnational governments that do not control the money supply (Musgrave 1959). However, some scholars have found that states still pursue such countercyclical policies (e.g., Brace 1991; Gramlich 1987; Husted, Morton, and Waller 1992).

Some economists have asserted that Keynesianism provided governments with an academic justification for deficit spending even after recession ends (Buchanan and Wagner 1977). Rowley (1987) and Buchanan (1987) suggested that Keynesianism and the repeated use of deficits attacked norms and cultural traditions that had long promoted fiscal responsibility. In doing so, the Keynesians also attacked conventional "Victorian" morality and encouraged an ethic of immediate demand gratification that has become widely adopted by the late twentieth century.[1] If this sort of cultural change has occurred, we may observe predictable consequences for fiscal policy at all levels of government, not just for the national government.

Despite Keynesian arguments justifying deficits, public concern over borrowing has not completely abated. What has troubled the public more than government borrowing, however, has been the burden of taxes. Particularly during the 1970s, taxpayer revolts took place in many states, often resulting in legal limitations on taxes and, at times, on expenditures by state governments. The effect of these restrictions on public debt is uncertain. Some state government leaders may avoid taxing and expenditure limitations by creating off-budget agencies and enterprises with borrowing authority. Most restrictions on taxing and spending apply to on-budget government activities, and most restrictions on debt apply only to general-obligation borrowing, so spending by off-budget entities funded by nonguaranteed debt may still be used. Most of the extant research on these legal limitations suggests that they do not have much impact on total state debt (see, e.g., Cox and Lowery 1990; Heins 1963; Nice 1991; but see also Bowler and Donovan 1993; Kiewiet and Szakaly 1992). However, research by Alt and Lowery (1994) indicates that balanced budget requirements do affect how state governments respond to short-term revenue shocks.

Keynesian arguments aside, fiscal capacity and economic necessity are both factors that should affect government borrowing (Bahl and Duncombe 1993). States that wish to borrow must demonstrate creditworthiness before their bonds are marketable. This requires some projection of the state's fiscal well-being into the future. It does not, however, necessarily require that a state currently be wealthy or high in revenue. Of course, wealthy states may also have little need to borrow unless they have extraordinary infrastructure requirements.

Political circumstances may also affect how closely policymakers follow norms of a balanced budget. Elected officials, for example, may wish to claim credit for new public projects begun during election years while postponing the repayment period until after the election. Thus a "political business cycle" in debt financing may occur (see Berry and Berry 1992; Nordhaus 1975). This trend may be exaggerated when incumbent elected officials are politically threatened, as might be the case when party competition is high (Alesina and Tabellini 1990; Baber and Sen 1986; Clingermayer 1991). Interparty competition has often been said to lead to higher state government spending, particularly of a redistributive sort, because candidates for office use public benefits to appeal to constituencies to win close elections (see, e.g., Dawson and Robinson 1963; Key 1951; for comparable arguments in the crossnational literature, see Roubini and Sachs 1989; but cf. Schick 1993). This could, of course, lead to more borrowing if the spending is not financed through additional taxes. Some researchers (e.g., Nice 1991) have also argued that state ideology affects borrowing decisions, with more liberal states tending to issue more debt.

Relatedly, political conflict within and among political institutions may also cause increased debt. McCubbins (1991) and Kiewiet and McCubbins (1991) claim that the federal government's fiscal policy is a product of joint action by the legislative and executive branches, with Congress having the more important power of enacting appropriations and approving taxes while the president holds the negative power of the veto. When different political parties with different fiscal preferences control the different branches, agreement on both spending and taxes is often lacking. The dollar totals approved for taxing and spending may differ, with a sizable deficit (or conceivably, a big surplus) resulting. Such deficits resulted in the United States during the 1980s when there was divided party control of the branches and the two legislative chambers. Of course, many observers argue that such deficits could also have occurred under unified governments, particularly when the party in power simultaneously favors tax cuts and increases in at least some categories of spending (Fiorina 1992). Research on the fiscal effects of divided government at the state level is just beginning (e.g., see Alt and Lowry 1994; Kiewiet and Szakaly 1992; Poterba 1994).

Other factors affecting state governments' willingness to borrow money should include interest rates and the federal government's tax treatment of debt. Both private and public borrowers should prefer to borrow when interest rates are low and forgo borrowing when rates rise. Subnational governments largely owe their place in the bond market to the federal tax exemption given to interest on their securities. Surprisingly, research on subnational debt has devoted little attention to these factors. Poterba's (1986, 1989) research on federal tax effects upon yield spreads between taxable and nontaxable securities demonstrates the relevance of tax law changes to government securities markets, but to our knowledge, no one has studied impacts upon total subnational borrowing.

One set of tax code changes likely to have affected borrowing by states was the federal Tax Reform Act (TRA) of 1986. This legislation contained many provisions on the tax treatment of state bond issues. By lowering individual marginal tax rates, tax exempt securities became less attractive for high-income individuals. The TRA included income from tax-exempt bonds in the calculation of the corporate alternative minimum tax, thus reducing the desirability of tax-exempt securities for corporate investors. The legislation broadened the definition of "private activity" bond issues and then reduced the statewide volume caps for private activity bonds—effective in 1988—that were imposed earlier in the 1980s. The new tax law also tightened the targeting requirements for mortgage bond issues and restricted arbitrage opportunities for proceeds derived from tax-exempt offerings (see Zimmerman 1991).

On the other hand, federal and state governments could also encourage subnational borrowing through the stimulative effect of intergovernmental aid. Many scholars have claimed that programs of the federal government have not only supplemented local programs but also stimulated their growth. Stimulation results from matching grants and formula-based categorical aid that rewards local governments willing to contribute on their own to a particular kind of service or activity. Federal programs also aid in the creation of new constituencies and bureaucracies that will lobby on their own for more state and/or local spending (Garand 1988; see also Stein 1984). Little if any attention has been paid to how intergovernmental aid affects subnational borrowing, but since many federal aid programs support capital projects (e.g., new roads, bridges, sewer systems, water treatment plants, each of which is traditionally financed by debt) it is likely that federal money would have some impact upon state borrowing.

DATA, MEASUREMENT, HYPOTHESES, AND METHODS

We propose that state debt financing can be explained as a function of two broad sets of influences, one economic and the other consisting of political and institutional factors. Within these two broad sets of influences, we can place all of the preceding explanations. However, we should emphasize that this separation between the two categories is entirely artificial, a simplifying device for purposes of discussion. Economics and politics are inextricably intertwined at all levels of government.

The data for our analysis are a pool of annual time series for 48 states running from 1961 through 1989.[2] Pooling enables us to compare the states for variations due to fixed differences at one point in time. It also enables us to evaluate the dynamics within states as debt-financing institutions respond to temporal factors causing variations in their behavior.[3]

The dependent variable for our analysis is the annual change in per capita net long-term state debt measured in constant 1982 dollars.[4] This measure includes both guaranteed and nonguaranteed debt and also encompasses debt issued by both state public authorities and the central governments within each state.[5] We make the measure per capita to control for differences between states due solely to disparate state population sizes. The data are measured in constant dollars to control for inflationary trends. However, even with this control for inflationary trends, the data are still nonstationary.

Debt levels per capita do not have a stable equilibrium but trend even after considering inflation. Of course, from a theoretical standpoint this nonstationarity makes sense. Unless states make significant progress in retiring their debts, debt levels are by definition cumulative since last year's debt constitutes the bulk of the current year's debt.[6] Therefore, we provide the state time series with stable equilibria by differencing prior to the analysis. Substantively, the result is that we model growth in state debt, rather than levels of state debt.

Fixed and Dynamic Economic Factors

Past research has often failed to control for economic factors that should affect state debt financing, perhaps because most studies have been cross-sectional and, therefore, static in nature. However, the economic determinants of state debt financing have both a static and a dynamic dimension. In a pool such as with our data, these dimensions are equivalent to the cross-sectional and time series dimensions. The variables described in our analysis as fixed economic are included to control for economic differences among states (i.e., the fixed effects in the pool). The variables described as dynamic economic are included to control for changing economic factors through time (i.e., the temporal effects in the pool).

In the fixed economic dimension we hypothesize that differences *among* states in annual borrowing are a function of the current incentives toward borrowing and lending. Several factors should be important here, including the income and revenues available to the state. With regard to income, we assert that high-income states should be more willing to incur new debt than low-income states because of their greater ability to handle new debt. Just as high-income families typically have greater willingness to incur new debt, so also should high-income states have a similar tendency. Such states have more positive expectations about future revenues that are necessary to retire debt. They also present a lower risk for lending institutions. Thus, income differences among states should affect corresponding differences in debt financing.

Of course, the preceding argument implies that current revenue levels should also affect growth in state debt. However, the expected direction of this relationship should depend on the type of revenues discussed. High own-source state revenues should produce a greater ability for states to incur larger debts, just as with high-income states. However, high intergovernmental revenues (i.e., revenues derived from either federal or local governments) may produce effects that run in either direction. In fact, the direction of the relationship tests specific theoretical issues already discussed. If intergovernmental revenues *substitute* for state monies, high intergovernmental revenues will result in lower state debts. On the other hand, if intergovernmental revenues *stimulate* state spending, then this should increase state debts. To control for these fixed economic factors, we include in the model each state's annual income and revenues, both own-source and intergovernmental, measured in per capita constant dollars.

These three variables should explain current time period differences among states in their relative propensity to debt finance. However, none of these factors captures the long-term time dynamic of responses by state institutions to changing economic conditions. Past economic conditions and revenues should affect the *need* for states to debt-finance. Tax bases that have been robust and growing should have less need to debt-finance than those which have been weak and shrinking. The effects of past economic conditions and revenues on the need for states to debt-finance should be cumulative and potentially involve substantial delays in developing. Because of the extended and potentially complex time dynamic, we use a distributed lag to model these effects.

We include three variables of this type in the model. Change in state economic conditions is measured as the annual change in per capita state personal income in constant dollars. Growth in state own-source revenues is the change in own-source constant-dollar revenues per capita. Intergovernmental revenue growth is the change in intergovernmental revenues per capita, also in constant dollars. Because it may take a long time for these effects to develop, we include nine annual lags for each of these variables.

Past economic growth and changing revenues should be one determinant of state debt financing. However, several other equally dynamic, but shorter-term, economic factors should also be important. If states are rational debt managers, then we would expect them to borrow less when interest rates are high and more when they are low. There are, of course, constraints on the ability of states to choose strategically when borrowing occurs. Still, most states have the ability to delay long-term debt through various means. They may increase their use of short-term debt; they may borrow from internal sources; or they may even engage in creative accounting practices.

To capture the relatively contemporaneous effects associated with this potential strategic behavior, we include in the model the current year's interest rate measured as the Bond Buyer's 20 index.[7] However, this models only one part of the short-term time dynamic. State officials often postpone new long-term debt by incurring short-term issues, but they may not be able to postpone in indefinitely. Unless they are beneficiaries of a windfall, these postponements are later transferred into new long-term debt.

We can capture these movements from short-term into long-term debt by including a variable to reflect these transfers. For this purpose, we use a distributive lag of growth in short-term debt per capita measured in constant dollars. Thus, increased growth in past short-term debt should produce increased growth in current long-term debt. Since these processers occur over a shorter time span than the economic variables already discussed, we include only six annual lags in the model.

One additional dynamic economic factor federal tax policy should also be considered. The TRA of 1986 altered incentives to purchasers of state bonds by removing tax exemptions for many "private activity" bonds. This should have reduced states' ability to obtain new debt. To represent this effect, we coded a dummy variable 0 for years before the effective date of the 1986 tax reform legislation and 1 for years after.

Political and Institutional Factors

The fixed and dynamic economic factors we have discussed constitute a baseline against which we can test hypotheses about the effects of various political and institutional factors on state debt financing. Specifically, scholars have argued that political-cultural factors, interparty competition, divided government, electoral cycles, and other institutional features (i.e., financial centralization, taxing, spending, and debt limitations) should also be important. We now describe the operationalization of these features into the model.

As noted, some economists have argued polemically that the Keynesian era resulted in a political-cultural norm toward increased debt financing. If a *culture of debt* exists in the United States, however, then it should also be present at the state level. To test this culture-of-debt hypothesis for the states, we include the annual federal debt measured in constant dollars.

Of course, there may also be state-specific political-cultural features that affect a state's propensity toward debt financing. States may differ based on an innate proclivity, a sort of local culture-of-debt hypothesis. Political science scholars have long theorized that political culture is an important determinant of state expenditures in various policy arenas. For example, Kincaid and Lieske (1991) cite approximately a hundred different studies that have subjected the political culture hypothesis to empirical testing. Various classification schemes have also been developed through the years to measure political culture (e.g., see Elazar 1984; Lieske 1993; Luttbeg 1971; Morgan and England 1987; Wright, Erikson, and McIver 1985).[8]

The classification scheme which seems most appropriate for our study is that developed by Wright, Erikson, and McIver (1985).[9] Aggregating over 76,000 state respondents from national public opinion surveys from 1974 through 1982 into a single measure, they classified states according to the opinion-liberalism of their citizens. We recode their continuous scale, making it run from most conservative (−1) to most liberal (+1). We include this measure with the expectation that more liberal states should covary with more rapidly growing state debts.

Various institutional features of state systems may also be important in explaining state debt. For example, past literature has suggested that electoral competition is relevant to state debt financing (Baber and Sen 1986; Clingermayer 1991; Lowery 1985; Nordhaus 1975). Some scholars have observed using cross-sectional data that the interaction between interparty competition and elections results in a systematic variation in state debt financing (Baber and Sen 1986). Others have argued simply for a "political business cycle" effect. Following these arguments we create a variable to measure electoral competition. First, we created a dummy variable to reflect the diverse gubernatorial election cycles of the 50 states. Then we coded a measure of interparty competition taken as 1 minus the absolute value of the average annual proportionate partisan majority in the chambers of the state legislature. The electoral competition variable was the interaction between the election year and interparty competition variables.[10]

Another institutional feature that may affect the propensity of states to incur debt is divided government. As noted in the previous section, divided government, which is yet another reflection of electoral competition, has been proposed as an explanation for the rampant growth in federal deficits through the 1980s. State governments are a convenient laboratory in which to test this notion because of the larger N. We measured divided government as a set of dummy variables coded for various combinations of gubernatorial and legislative partisanship.[11] If the divided-government hypothesis is correct then we should see higher debt growth in states with divided partisan institutions, but lower debt growth under conditions of unified government.

It is also plausible that fiscal centralization could result in more state debt. Some states centralize policy responsibilities, while others allow local governments to be more active. To control for this institutional difference we included a measure of fiscal centralization, which was the ratio of state revenues to both state and local revenues.[12] Greater centralization should lead to higher growth in state debts.

Finally, taxing, spending, and debt limitations imposed through state constitutional or statutory channels may produce constraints on the growth of state debt. If these constraints are effective, then those states that have such limits should have more slowly growing debts than those which do not. The imposition or removal of such limits through time should also result in changing state debts. However, for taxing and spending limits there may also have been unanticipated effects. For example, the taxpayer revolts of the 1970s, which resulted in tax rate limits and, in some cases, spending caps, could actually have increased debt financing. Politicians, eager to distribute benefits to citizens, may have bypassed these limitations through alternative funding methods like increased debt.

To evaluate the effect of taxing, spending, and debt limitations on state debt we coded two dummy variables. The first, which includes only taxing and spending limitations, switches on for those states and years in which states had such limitations and off for all other states and years. The second variable, which includes debt limitations is coded 1 for those states and years in which there were absolute-dollar-amount or flexible limits on levels of state debt.[13] Tax, spending, and debt limitations should decrease state debt, but for the stated reasons this is by no means a certainty.

RESULTS

Table 1 contains regression estimates for each of the variables discussed. Figure 1 graphs the impulse response functions for each of the four distributed lag variables. Impulse response functions are just plots of the conditional probability of a response to variable k at some lag, given a finite response distribution. Let us first consider the effects of the economic variables on growth in state debt financing. The model shows that such growth is a function of both fixed and dynamic economic factors.

Fixed Economic Factors

Along the fixed economic dimension, income and revenue levels produced differences in state debt

TABLE 1

Determinants of State Debt Financing, 1961–89

EFFECT TYPE AND INDEPENDENT VARIABLE	ESTIMATED COEFFICIENT	STANDARD ERROR
Economic attributes		
Fixed effects		
Per capita income	.007***	.002
Per capita revenues	.130***	.020
Per capita IGR	.078*	.043
Dynamic effects		
Economic growth[a]	−.110***	.024
lag 1	.007*	.004
lag 2	−.006*	.004
lag 3	−.015***	.036
lag 4	−.021***	.038
lag 5	−.024***	.038
lag 6	−.023***	.034
lag 7	−.019***	.027
lag 8	−.011***	.017
lag 9	.000	.001
Growth in state revenues[b]	−1.167***	.190
lag 1	−.120***	.028
lag 2	−.157***	.026
lag 3	−.179***	.029
lag 4	−.186***	.031
lag 5	−.179***	.032
lag 6	−.157***	.029
lag 7	−.119***	.023
lag 8	−.068***	.140
lag 9	−.001	.007
Growth in IGR[c]	−1.319**	.511
lag 1	−.151**	.068
lag 2	−.183***	.066
lag 3	−.201***	.076
lag 4	−.205**	.085
lag 5	−.194**	.087
lag 6	−.169**	.080
lag 7	−.130**	.065
lag 8	−.076*	.042
lag 9	−.008	.030
Growth in short-term debt[d]	1.123***	.389
lag 1	.268***	.071
lag 2	.236***	.063
lag 3	.203***	.062
lag 4	.171**	.069
lag 5	.139*	.082
lag 6	.107	.099
Bond Buyer's 20 index	−2.950***	.913
1986 federal tax reform	−184.430***	9.151
Political and institutional attributes		
Federal debt	.366E-07***	.073E-08
Opinion-liberalism	50.462**	20.400
Electoral competition	4.822**	2.456
Rep. gov./Dem. legis.	2.572	3.162
Dem. gov./Rep. legis.	−7.932**	3.429
Rep. gov./Rep. legis.	−3.418	3.453
Split legislature	−7.600	4.786
Financial centralization	−.004	.005
Taxing/spending limits	6.840*	4.051
Debt limits	−1.241	3.047
Constant	12.763*	7.148

Note: The dependent variable is the annual change in per capita net long-term state debt measured in constant 1982 dollars. Regression estimates are corrected for autocorrelation and heteroscedasticity as discussed in the Appendix. Significance tests are two-tailed with 1,392 degrees of freedom. [a] Mean lag = 5.45 years. [b] Mean lag = 4.23 years. [c] Mean lag = 4.19 years. [d] Mean lag = 3.00 years.
* $p \leq .10$. ** $p \leq .05$. *** $p < .01$.

financing in the expected direction. For income, each $1,000 change in per capita income produced about a $7 change in per capita state debt. Rich states acquired marginally greater debts; poor states acquired marginally lesser debts. As discussed, this could be because rich states are better able to afford debt than poor states; or it may be because they are a better lending risk. For whatever reason, income differences between states resulted in differentials in state borrowing.

The estimate for own-source state revenues is consistent with this same interpretation. Revenue differences resulted in differences between states in the growth of debt. Each $1,000 annual difference between states in per capita revenues resulted in about a $130 per capita difference in annual debt growth. That is, high-revenue states borrowed larger sums than low-revenue states (and vice versa) as expected.

For intergovernmental revenues, the result in Table 1 is consistent with the stimulation, rather than the substitution, hypothesis. Each $1,000 per capita difference between states in intergovernmental revenues resulted in about a $78 difference between states in debt growth. That is, controlling for size, income, and own-source revenue effects, states with higher intergovernmental revenues borrowed more money annually than those with lower intergovernmental revenues. This implies that intergovernmental revenues stimulate, rather than reduce, state demand for borrowing.

Dynamic Economic Factors

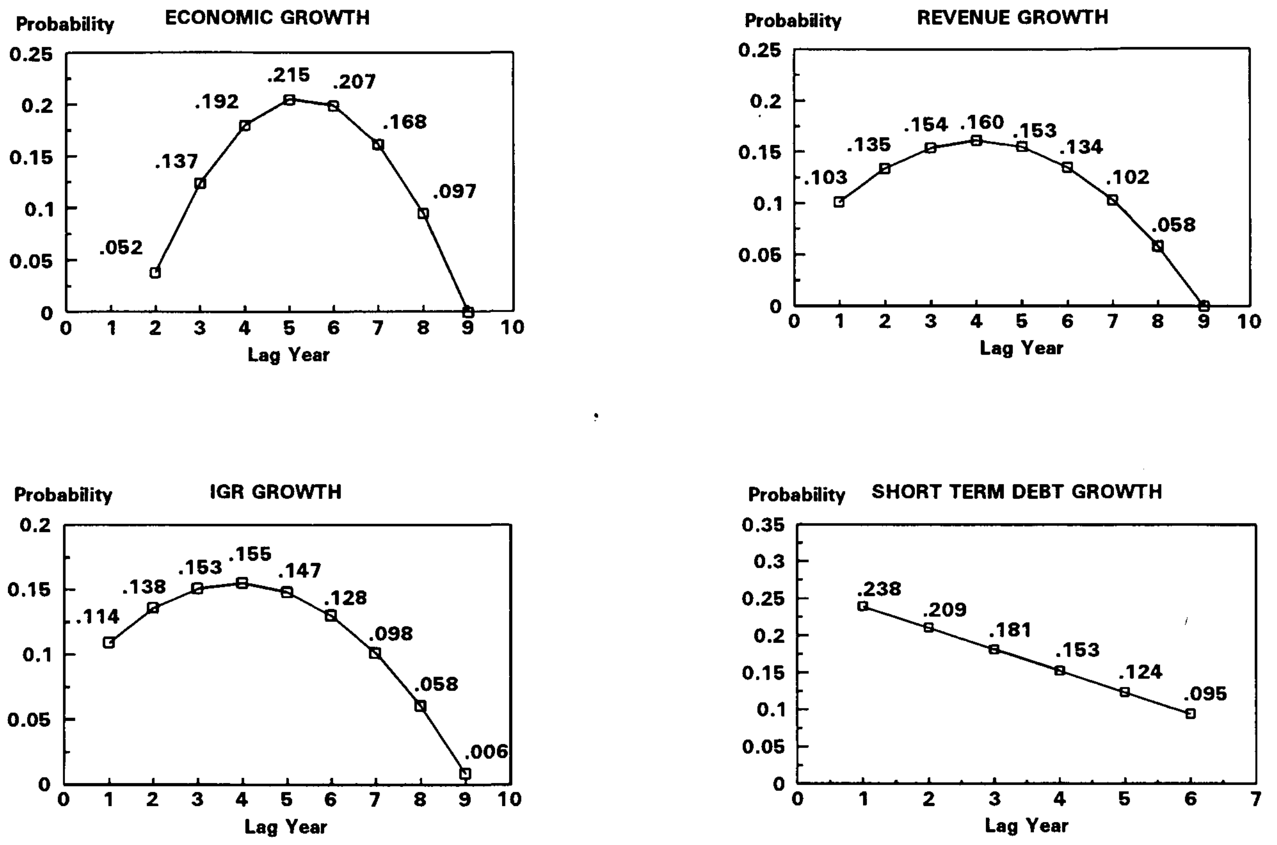
The preceding fixed economic effects on growth in state debt are interesting, but state debt occurs through time. So we must also control along the dynamic dimension for changes in the magnitude and direction of growth. The total magnitude of the response to a distributed lag variable is the sum of the lagged coefficients. Thus, for the economic growth variable, each $1,000 decrease in the growth of per capita personal income accumulated over the previous nine years produced about a $110 per capita increase in the growth of current state debt. In other words, a state economy that declined in the past had increasing state debt in the present (and vice versa).

Similar economic effects are also observed for the two revenue variables. Each $1,000 decline in own-source state revenues over the past nine years resulted in about an $1,167 increase in the growth of current state debt. Each $1,000 decline in per capita intergovernmental revenues over the past nine years resulted in about a $1,319 increase in growth in current state debt.

This long-term dynamic relation among income, revenues, and state long-term debt implies the obvious. When governments do not have the money to finance their continuing commitments through revenues, they typically turn to debt financing. This is a political problem at the national level, evidenced by the increased growth in the federal debt after the 1982 Reagan administration tax cuts. The analysis we have

FIGURE 1

Economic Variables and the Dynamics of Changing State Debt



reported confirms that it is also a problem at the state level, despite a different set of institutional incentives and constraints.

It is also interesting to observe the temporal patterns associated with these variables. One descriptor of this temporal pattern is the mean lag, which is just the average length of time it takes for a distributed lag stimulus to produce a change. Of course, we can also compute conditional probabilities of a response at each lag separately (as reported in Figure 1). As noted, the total response to a distributed lag variable is measured by the sum of the lagged coefficients. So the conditional probability of a response at any particular lag is the ratio of the coefficient for that lag to this sum.[14]

Figure 1 shows the particular probabilities associated with a change in state debt for each lag of change in state income. Thus, the probability of a decline in the current year's state debt resulting from an improvement in last year's state economy was zero. As shown in Table 1, a $1,000 growth in the state economy one year previous produced about a $7 per capita increase in current state debt. This effect is consistent with how fixed income differences between states affect state debt, as discussed. However, for all other lags the effect of economic growth on growth in state debt was in the opposite direction. If a decline occurred in the state economy two years ago, then there was a .044 probability of an increase in current state debt. A decline in economic growth five or six years ago produced about a .18 probability of an increase in current state debt. This probability declined to about .08 in the eighth year and to .00 in the ninth year. The mean lag (reported in Table 1) shows that the average length of time it took for a change in the state economy to affect change in state debt was about 5.45 years. This means that a relatively protracted time dynamic in changing state indebtedness is associated with past changes in a state's economy.

A more immediate, but somewhat flatter, time dynamic occurred for the larger effects of revenues on state debt financing. A decline during the previous year in state revenues from either own-source or intergovernmental sources increased the probability of new debt financing in the current year by about .10. The probability was relatively flat for the second through the fifth year, decaying gradually to zero during the ninth year. The mean lag for a response in state debt to declining own-source revenues was about 4.23 years; for declining intergovernmental revenues the average response time was about 4.19

years. Both response dynamics suggest that revenue collection for most of the past decade is a good predictor of state tendencies toward new debt financing.

The preceding evidence shows that states behave much like the national government in their acquisition of new debt. This is not to say, however, that states eagerly accept new debt. As argued, state-level actors are strategic in their responses to changing financial markets. As interest rates rise, they finance less through long-term debt; as they fall, they finance more. Consistently, each 1% change in the Bond Buyer's 20 index produced a change of about $2.95 per capita in the growth of state debt. This short-term influence is fairly modest, but it is also dwarfed by the delayed response due to debt postponements through short-term debt.

Increased interest rates should cause greater use of short-term debt, alternative funding methods, and creative accounting, which are later transferred into long-term debt. Consistently, Table 1 shows that each $1,000 in short-term debt accrued over the past six years resulted in about $1,123 in new long-term debt in the current year. The average delay for short-term debt transfers into long-term debt was about 3.00 years. Figure 1 shows that the probability of change induced by one-year-old short-term debt was about .24; the likelihood of change decayed linearly to about .1 by the sixth year after acquiring short-term debt. The immediate response to interest rates, coupled with these transferral effects, imply substantial variation in long-term debt due to the strategic economic behavior of governmental actors.

Finally, economic incentives to debt financing by states also flow from the federal government. The federal TRA of 1986 sharply altered incentives for state debt financing. The tax policy intervention produced a drop of about $184 per capita in the annual growth of state debt.

Political and Institutional Factors

Economic factors, some of which are driven by state and national politics, strongly affect changes in state debt financing. State institutions respond to incentives and constraints from the state's economy, financial markets, the revenue system, and national political institutions. We shall now report results on how various other political and institutional factors affect state debt financing. The economic variables already discussed provide a baseline against which we can test hypotheses relevant to these factors.

Concerning the national culture-of-debt hypothesis, the analysis reported in Table 1 provides evidence to support this phenomenon. As the federal debt grew from 1961 through 1989, growth in state debt changed consistently. Of course, this is not a definitive test, since it shows only the existence of a common pattern at both the national and state levels, rather than a culture of debt. However, we also cannot reject this notion based on the analysis.

Concerning the local culture-of-debt hypothesis, there is evidence that the opinion-liberalism of state populations affects the propensity of states to incur debt. A shift in state opinion-liberalism from neutral (0) to the most liberal position (+1) produced an increase of about $50 in the growth of per capita state debt. One standard deviation of the opinion-liberalism measure is about .082, which implies that states one standard deviation above the average state opinion-liberalism incurred about $4.10 more growth in debt per capita.

Do the political institutions of a state make a difference in the growth of state debt financing? We find no evidence that the degree of centralization of state financial institutions affects the growth of state debt.[15] States in which more revenue is collected by state than local governments had about the same growth in state debt as those in which the opposite was true. Thus, control of state finances by state-level actors does not seem to produce adverse incentives to the growth of state debt.

Additionally, there is no evidence from the results that divided government makes a difference to the growth in state debt. The only statistically significant effect (Democratic governor, Republican legislature) ran in the opposite direction from what the divided government hypothesis predicts. McCubbins (1991) asserts that the combination of a Republican chief executive and Democratic legislature results in "out of control" public debt. However, this is disconfirmed when tested in the more extensive laboratory of 48 states. State institutions characterized by various other combinations of the divided government hypothesis also showed no relation with the growth of state debt.[16]

Of course, this does not mean that political institutions are unimportant. The electoral competition variable in Table 1 weakly supports both the interparty competition and the "political business cycle" hypotheses observed by previous empirical research. Growth in state debts was higher in states with high interparty competition in the year preceding an election. In was lower in states and years where the opposite was true. This movement and timing suggest that when politicians are under electoral stress, incumbents of both political parties manipulate debt to assist in their reelection efforts. While the particular mechanisms of political control over state debt financing remain unclear, strategic political behavior is evident from the analysis.

Strategic political behavior in managing state debt is also weakly supported by the results for taxing and spending limitations. These limits are legal constraints on taxing and spending intended to control state-level politicians. Many resulted from the taxpayer revolts of the 1970s and 1980s, similar to Proposition 13 in California, so there is little doubt about their intended effect on politician behavior. However, we find that these limits may have produced an unanticipated effect. They increased the growth in state debt by about $6.84 per capita annually. This estimate is small and weakly significant at the .10 level, but the pattern suggests that state politicians used debt financing to their strategic advantage. Under revenue constraints, they sought alternative ways to maintain past levels of citizen benefits.

This adaptive behavior by politicians raises serious doubts about the efficacy of popular proposals for constitutional debt limitations in controlling public debt. Do debt limitations produce significantly lower public debt? Virtually all states have them in some form, but the analysis in Table 1 shows that they have no statistically significant impact on net increases in state debt.

CONCLUSIONS

We have examined the causes of public debt in the American states. Our purpose has been to increase understanding of the politics of public debt generally, as well as to provide evidence on state debt specifically. The analysis yields important insights on government borrowing that may help provide an understanding of the federal debt. It also challenges conventional wisdom on state debt, specifically questioning the popular belief that state financial institutions are insulated from the political forces and economic conditions believed to affect borrowing at the federal level.

Concerning the recent rampant growth in the federal debt, many partisans have proposed that the national government adopt constitutional or statutory tax, expenditure, and debt limitations such as now exist at the state level. However, our analysis shows that these features do not significantly alter growth in debt for state governments. Tax and expenditure limitations may actually increase growth in state debt, as self-interested politicians evade formal constraints through alternative means of financing. Likewise, debt limitation institutions have no effect in reducing the growth in state debt. The experience of states should be discouraging to those who would attempt to reduce the national debt through constitutional means.

The conventional wisdom that state governments are somehow fundamentally different from the national government because of separated capital budgets and balanced budget norms also appears erroneous. We found evidence of state debt financing that is consistent with the hypothesis of a national culture of debt. Growth in state debt tracks closely with the national debt, so states are not immune to "out of control" public finances. Additionally, we found evidence that state-specific political-cultural factors also affect their propensity to debt-finance. States with more liberal citizens have a higher propensity to incur debt, while just the opposite is true for states with more conservative citizens.

Our analysis also challenges recent research on the causes of state fiscal instability. For example, we find no support for the hypothesis that divided government causes greater growth in long-term state debt. Alt and Lowery (1994) report a relationship between divided government and end-of-year state government revenues and expenditures (see also Poterba 1994). In particular, they find that revenues for unified governments change more than revenues for split-branch or split-legislature governments when budget surplus or deficit develops. They conclude that this difference helps explain the persistence of state-level fiscal problems. Interestingly, their analysis does not consider state deficits or debt per se but only the short-term response of revenues and expenditures to changing budget surplus or deficits. Additionally, it should be noted that their analysis employs data that are nonstationary and (theoretically) cointegrated.[17] In contrast, our analysis of change in state debt produces no evidence that divided government affects state fiscal instability over the long term. We even see some very weak evidence that one kind of partisan division—that between a Democratic governor and a Republican legislature—may actually cause a slight decline in the growth of long-term state borrowing.

So what does determine public debt? Our analysis suggests that debt management by the states is a process shaped by economic conditions and strategic political behavior. States increase and decrease debt for reasons similar to self-interested consumers or private firms. In the short term, governments with high income and revenues can afford to take on higher debt, so they do. Investors rightly perceive them as a better credit risk and loan them money. Over the longer-term, governments that are sharply increasing in income and revenues have little need to borrow, so they do not.

Also, like individuals or private firms, governments are discouraged from borrowing when interest rates are high. The advisability of borrowing is also altered by economic incentives emanating from national political institutions. For example, we find that changes in federal tax law affect state governments, just as they do individuals and private firms. The 1986 federal TRA had the intent effect of slowing the growth of state government borrowing. Also, state governments with heavy short-term debt burdens later transformed these into long-term debt, just as individuals or private firms refinance their short-term borrowing.

Fiscal policymakers behave in ways consistent with the model of calculating, self-interested politics. Accordingly, circumstances and institutions also shape debt-financing decisions, presumably by providing political incentives for policymakers to borrow money. For example, partisan competition tends to increase state borrowing in the year before gubernatorial elections, consistent with at least one version of the political business cycle argument (see Baber and Sen 1986). This suggests that—irrespective of economic conditions—politicians see state debt as a way to enhance their electoral fortunes. Incumbents can claim credit for capital projects begun before elections, while the costs of debt-financed projects do not come due until long after the votes are tallied.

Similarly, self-interested policymakers often finesse political institutions intended to constrain fiscal behavior. As already discussed, the presence of tax and expenditure limitations may actually increase—rather than decrease—long-term borrowing. Constitutional limits on state debts also do not limit growth in state borrowing. Absolute dollar limits, flexible dollar limits, and public referenda and supermajority legislative vote requirements were all found to have no impact upon increases in long-term state debt. Just as at the federal level, when state-elected officials cannot increase taxes they use borrowing to provide funds for desired programs. While this does not seem out of character for the federal government (which regularly runs a deficit, rather than hike taxes), it runs counter to the conventional wisdom for state governments that are bound by balanced-budget expectations and constitutional limits on deficit financing.

These findings should cause a rethinking of conventional wisdom about public debt financing. State debt financing is largely shaped by the same types of influence that affect the national government, as well as individuals and private firms. Debt financing occurs when there is either the need or capacity for new debt. It is also affected by many of the same political influences that affect national political economies. Thus, the common belief that state government borrowing is unique is in serious doubt. Equally important, recent proposals for controlling the rampantly growing national debt in a manner consistent with state institutions seem fundamentally misguided.

APPENDIX

Precise descriptions of the variables used in our analysis are as follows:

ECONOMIC ATTRIBUTES

Fixed Effects

Per capita personal income: average per capita personal income in each state and year measured in constant dollars.
Per capita own-source state revenues: revenues per capita from state level sources in constant dollars.
Per capita intergovernmental aid: intergovernmental revenues per capita in constant dollars.

Dynamic Effects

Economic growth: the change in per capita personal income in each state and year measured in constant dollars.
Growth in own-source state revenues: change in revenues per capita from state level sources in constant dollars.
Growth in intergovernmental revenues: change in intergovernmental revenues per capita in constant dollars.
Growth in short-term debt: change in short-term debt per capita in constant dollars.
Bond Buyers 20 index: an inflation adjusted index reflecting the average annual interest rate charged to state governmental and municipal borrowers.
1986 federal tax reform: a dummy variable coded 1 for the effective date of the 1986 federal tax reforms.

POLITICAL AND INSTITUTIONAL ATTRIBUTES

Federal debt: the U.S. government debt in constant dollars.
Opinion-liberalism: Wright, Erikson, and McIver's (1985) measure of state ideology recoded to reverse coefficient signs. We use their unweighted mean scores, with the correction for Nevada (ibid., tbl. 3 and n. 5).
Divided government: A set of dummy variables coded to reflect the partisan divisions of state government. (For example, Republican governor/Democratic legislature was coded 1 for when the governor was Republican and legislature was Democratic.) Unified democratic party control was retained in the intercept.
Electoral competition: the interaction of interparty competition and gubernatorial election year. Interparty competition is defined as 1 minus the absolute value of the average proportionate partisan majority for each year in the chambers of the state legislature. Election year is a dummy variable coded 1 for the year before a gubernatorial election and zero for all other years.
Financial centralization: the ratio of state general revenues to state and local general revenues.
Taxing and spending limits: a dummy variable coded 1 for states and years in which taxing and spending limits existed.
Debt limits: a dummy variable coded 1 if the state has either a fixed or flexible limitation on the amount of accruable debt.

Relations between all of these variables and change in state debt were estimated using the pooled time series cross-section method discussed in Greene (1993, 447–59). The technique employs a generalized least squares transformation matrix to improve estimation efficiency which is limited by residual autocorrelation, heteroscedasticity, and mutual unit correlation. Designing an appropriate generalized least squares transformation matrix requires first obtaining ordinary least squares estimates. Based on residual diagnostics from ordinary least squares, the model was estimated under assumptions of different autocorrelations and variances for each state but not mutual unit correlation. (Note that we did not use the Parks procedure in estimating coefficients or standard errors.)

We diagnosed ordinary least squares versions of the model using the pooled Durbin-Watson statistic (for autocorrelation) and Lagrange Multiplier tests (for heteroscedasticity and mutual unit correlation). The pooled Durbin-Watson statistic is just the average of the Durbin-Watson d-statistics for each state time series. Details of the tests for heteroscedasticity and mutual unit correlation can be found in Greene (1993, 448–55). The pooled Durbin-Watson statistic was 1.47, indicating that moderate amounts of autocorrelation remained after differencing the dependent variable. The average rho for the pool was .27, but the estimation procedure we used employed a different rho for each cross section. The LaGrange multiplier statistic for heteroscedasticity was 880.06 (χ^2 with 47 degrees of freedom), indicating diverse residual variances. The LaGrange multiplier statistic for mutual unit correlation (χ^2 with 2,256 degrees of freedom) indicating cross-sectional correlation. We did not, however, correct for mutual unit correlation. The average correlation between cross sections was $r_{ij} = .19$. Monte Carlo evidence suggests that for this level of mutual unit correlation there is little or no efficiency gain from the correction (Beck and Katz 1993). Additionally, the standard errors may be suspect from the traditional SURE approach to this correction.

Because we wanted the model to reflect the dynamics of the time dimension of state debt financing, we estimated the pooled model as a finite distributed lag. A common problem with the distributed lag approach is inefficient estimation due to collinear lags and lost degrees of freedom. Extreme multicollinearity can also produce unstable and meaningless estimates. To ameliorate these difficulties, we employed the methodology developed by Almon (1965). The Almon model in the pooled time series cross-section context can be written:

$$Y_{st} = \beta_0 + \sum_{k=1}^{K} \delta_{jk}(L)\beta_k X_{kst} + \varepsilon_{st}$$

$$j, 0 \ldots J$$
$$k, 1, \ldots K,$$

where Y_{st} is the per capita change in constant-dollar state debt for state s at time t, X_{kst} is independent variable k for state s at time t, β_k is the coefficient for variable k, δ_{jk} is a set of lagged response weights for each lag j of variable k, L is a polynomial in backshift algebra specifying the lag response structure (e.g., $L = 1 + L + L^2 + \ldots L^J$), and ε_{st} is a vector of errors. The Almon approach restricts the lag response structure to lie on a polynomial of degree less than the total number of lags included in L. The researcher must select the appropriate polynomial degree, as well as an upper lag length based on theoretical, empirical, or statistical criteria.

We used both theoretical and statistical criteria to choose the upper lag length for the economic variables. Generally, we knew that the lag length for economic growth could be longer than for own-source and intergovernmental state revenues. With this as a starting point, we used Schwarz's (1978) criterion to establish the polynomial degree and appropriate lag lengths for the four dynamic variables. The polynomial for the economic variables was second-degree; it was first-degree for the short-term debt variable. The final model specification included nine lags for the economic variables and six lags for the growth in short-term debt variable.

Results from estimation were very robust to the addition or deletion of variables or of states from the analysis. The signs and magnitudes of coefficients were unchanged even if we kept only the economic variables or only the institutional/cultural variables in the model. Additionally, we did multiple split-sample robustness tests in which the sample was divided in half and reestimated. As another diagnostic, we tested for remaining fixed effects in the pool by adding dummies for each state into the final model. The F-change statistic associated with the dummy variable model was $F_{1,1391} = .14$. The variance of the estimate for the final model was about ±$27 per capita debt growth per state-year. The final model omits Alaska and Hawaii in order to accommodate the nine annual lags preceding the start of the data. However, we should note that the analysis was also robust with Alaska and Hawaii included but using a restricted sample size.

Notes

1. Some observers have questioned the relevance of Keynesianism for debt burdens, primarily because peacetime government borrowing in the United States did not begin until long after Keynes was dead (Tullock 1987). One might also wonder whether Keynesianism followed rather than caused cultural change that made debt financing more accepted.

2. We omit Alaska and Hawaii from the analysis because of the nonavailability of data for lagged observations. The analysis starts in 1961, but with lagged values for some variables running back to 1952. These states were not states through this period, so they were excluded from the analysis.

3. It should be noted that the pooled time series cross-section data are neither time-series- nor cross-section-dominant, which has important implications for statistical estimation. Cross-section-dominant pools are often estimated using fixed or random effects models, while ignoring problems of autocorrelation. Time-dominant pools typically have more serious autocorrelation problems (similar to those associated with a single time series). Here we must consider both problems by controlling for fixed differences spatially, as well as modeling the dynamics in each cross section.

4. All variables used in our analysis are precisely defined in the Appendix.

5. Ideally, we would have liked to produce a measure which is identical to the federal deficit measure. However, different institutions and accounting practices made this impossible. The federal deficit is just the difference between revenues and expenditures during a fiscal year. Due to the fiscal obligation process, actual federal expenditures are spread out over several years after an initial budget is approved. States also use an obligation system, but the details of this process differ between states and federal government. Also, since there is no separation between capital and operating budgets at the federal level, these expenditures include both categories. At the state level, operating budgets must technically balance, which is untrue at the federal level. However, state governments do in fact often operate in the red by approving projects to be financed through long-term borrowing. This is not calculable as a difference between revenues and expenditures, since bond issues are not reported as expenditures. By considering the annual *change* in state long-term debt, both guaranteed and nonguaranteed, we provide the most comparability that is possible between state and federal measures.

6. Statistical tests confirm that levels of state debt are nonstationary. We did graphs and autocorrelation plots for each state, which were indicative of nonstationarity in all states. We also computed a pooled augmented Dickey-Fuller test. The average Dickey-Fuller test statistic was −2.44 (from an auxiliary regression using both a constant and a trend term). With a critical value of −3.66 (from Dickey and Fuller 1981, 1063) we were, therefore, unable to reject the null hypothesis of a unit root for the entire pool. Regressions with nonstationary data are well known to result in theoretically infinite error variances and spurious regressions (Granger and Newbold 1974).

7. This is an inflation-adjusted index made up of interest calculated from a variety of municipal bond issues of various grades and maturities. It is a desirable measure, since it would fluctuate in accordance not only with prevailing commercial interest rates but also with changes in federal tax law affecting state and local borrowing. We also tested measures other than the Bond Buyer's 20 index, including the prime interest rate and the long-term interest rate on U.S. government bonds. It did not generally matter which measure we used in model specifications. Early on, we considered using a measure that differentiated between states based on bond ratings. *Moody's* rates bond issues according to the financial health of the institution issuing the bonds. We rejected this measurement strategy for three practical reasons: (1) it does not rate many bond issues by state institutions; (2) it rates individual bond issues, not states during particular years, so that valid measurement was impossible without information on the particular bond issues made by each state during each year; and (3) it gives different ratings to guaranteed versus nonguaranteed bonds (since we were considering a measure that included both, some sort of weighting scheme would have been needed to obtain a rating). Given those various intractable problems, we discarded bond ratings as a viable measurement option.

8. Lieske (1993) summarizes these measurement schemes and offers a critique of this literature. He presents his own measurement scheme, which uses cluster analysis to categorize geographic subunits of the U.S. down to the county level. We explored the possibility of using Lieske's county-level data to measure state political culture but concluded that there were serious weighting and validity problems involved.

9. None of the alternative measures of political culture is entirely suited to pooled time series cross-sectional analysis. This is because they are all cross-sectional, varying between states but lacking a time varying component. Wright, Erikson, and McIver's (1985) measure also does not have a time varying component. However, it does measure state opinion-liberalism across multiple years on a factor that many argue is relatively time invariant. We should note also that a previous version of this study included a surrogate for state liberalism, Aid for Families with Dependent Children recipients, which was time-variant and produced statistically significant results. However, this measure was deemed too indirect, so we chose to include the Wright, Erikson, and McIver measure in the final version.

10. We call this measure electoral competition. However, it does not necessarily measure the competitiveness of state elections. For example, a party that controls 51% of the state legislature may have won by 100% of the vote in those districts where elections were held. See Holbrook and Dunk 1993 for a detailed consideration of this issue.

11. Nebraska and Minnesota, prior to 1972, had nonpartisan elections and unicameral legislatures. These conditions are captured through the regression intercept, along with

unified Democratic governments. Note that in a different analysis we coded and included a separate dummy variable to reflect Nebraska and Minnesota prior to 1972. It had no effect on the coefficients or standard errors in the reported model. Therefore, to conserve space and reported coefficients we included this condition in the intercept.

12. We also tried the ratio of state government employees to state and local government employees. The two variables produced almost identical results and the choice of the financial variable is entirely arbitrary.

13. The taxing and spending limits variable was constructed from information in *Significant Features of Fiscal Federalism* (Advisory Council on Intergovernmental Relations 1992, 14–17). The debt limitation variable was constructed through examination of the most recent copies of each state constitution and the accompanying historical notes. We also coded a debt limit variable for whether states had a referendum or legislative supermajority requirement. However, it was necessary to choose between these variables for the analysis. The dollar and flexible limits were more robust and were therefore included in the model.

14. A more rigorous mathematical discussion of impulse response functions is in the Appendix.

15. The measure employed here was the ratio of state general revenues to state plus local general revenues. However, we also tried the ratio of state employees to state plus local employees. There was no statistical difference in the reported result.

16. We should note here that we also tested hypotheses about the relationship between institutional partisanship and growth in state debt. Collinearity problems prevented inclusion of these variables in the same model with the divided government variables. However, it is worth emphasizing that separate estimations also showed no relation between the partisanship of state institutions and state debt.

17. Integrated time series data are well known to produce standard errors that are too small (Granger and Newbold 1974). Regressions run on cointegrated data result in incorrect standard errors due to the "superconvergence" properties of the least squares estimator (Banerjee et al. 1986; Stock 1987).

References

Advisory Council on Intergovernmental Relations. 1992. *Significant Features of Fiscal Federalism*. Washington: GPO.

Alesina, Alberto, and Guido Tabellini. 1990. "A Positive Theory of Fiscal Deficits and Government Debt." *Review of Economic Studies* 57:403–14.

Almon, Shirley. 1965. "The Distributed Lag between Capital Appropriations and Expenditures." *Econometrica* 33:178–96.

Alt, James E., and Robert G. Lowry. 1994. "Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States." *American Political Science Review* 88:811–828.

Anderson, Gary M. 1987. "The U.S. Federal Deficit and National Debt: A Political and Economic History." In *Deficits*, ed. James M. Buchanan, Charles K. Rowley, and Robert D. Tollison. New York: Basil Blackwell.

Baber, William R., and Pradyot Sen. 1986. "The Political Process and the Use of Debt Financing by State Government." *Public Choice* 48:201–15.

Bahl, Roy, and William Duncombe. 1993. "State and Local Debt Burdens in the 1980s: A Study in Contrast." *Public Administration Review* 53:31–40.

Banerjee, A., J. Dolado, D. Hendry, and G. Smith. 1986. "Exploring Equilibrium Relationships in Econometrics through Static Models: Some Monte Carlo Evidence." *Oxford Bulletin of Economics and Statistics* 48:253–77.

Barro, Robert. 1979. "On the Determination of Public Debt." *Journal of Political Economy* 87:940–71.

Beck, Nathaniel, and Jonathan Katz. 1993. "The Analysis of Cross-national Panel and Related Models." Presented at the annual meeting of the American Political Science Association, Washington.

Berry, Frances Stokes, and William D. Berry. 1992. "Tax Innovation in the States: Capitalizing on Political Opportunity." *American Journal of Political Science* 36:715–42.

Bowler, Shaun, and Todd Donovan. 1994. "Is Direct Democracy Good for Public Budgets? The 'Constitution' of State Indebtedness." Presented at the annual meeting of the Western Political Science Association, Albuquerque.

Brace, Paul. 1991. "The Changing Context of State Political Economy." *Journal of Politics* 53:297–317.

Buchanan, James M. 1987. "Budgetary Bias in Post-Keynesian Politics: the Erosion and Potential Replacement of Fiscal Norms." In *Deficits*, ed. James M. Buchanan, Charles K. Rowley, and Robert D. Tollison. New York: Basil Blackwell.

Buchanan, James M., and Richard E. Wagner. 1977. *Democracy in Deficit: The Political Legacy of Lord Keynes*. New York: Academic.

Clingermayer, James C. 1991. "An Intergenerational Transfer Model of State Debt Financing." *Public Choice* 72:13–21.

Cox, James, and David Lowery. 1990. "The Impact of the Tax Revolt Era State Fiscal Caps." *Social Science Quarterly* 71:492–509.

Dawson, Richard E., and James A. Robinson. 1963. "Interparty Competition, Economic Variables, and Welfare Policies." *Journal of Politics* 25:265–89.

Dickey, D. A., and W. A. Fuller. 1981. "Likelihood Ratio Statistics for Autoregressive Time Series with a Unit Root." *Econometrica* 49:1057–72.

Elazar, Daniel J. 1984. *American Federalism*. 3d ed. New York: Harper & Row.

Fiorina, Morris. 1992. *Divided Government*. New York: MacMillan.

Garand, James. 1988. "Explaining Government Growth in the U.S. States." *American Political Science Review* 82:837–49.

Gramlich, Edward M. 1987. "Subnational Fiscal Policy." In *Perspectives on Local Public Finance and Public Policy*, ed. John M. Quigley, vol. 3. Greenwich, CT: JAI Press.

Granger, Clive W. J., and Paul Newbold. 1974. "Spurious Regressions in Economics." *Journal of Econometrics* 2:111–20.

Greene, William H. 1993. *Econometric Analysis*, 2d ed. New York: Macmillan.

Heins, A. James. 1963. *Constitutional Restrictions against State Debt*. Madison: University of Wisconsin Press.

Holbrook, Thomas M. and Emily van Dunk. 1993. "Electoral Competition in the American States." *American Political Science Review* 87:955–62.

Husted, Thomas A., Rebecca B. Morton, and Christopher J. Waller. 1992. "Political Business Cycles in State Economies." Presented at the annual meeting of the American Political Science Association, Chicago.

Key, V. O., Jr. 1951. *Southern Politics*. New York: Alfred A. Knopf.

Kiewiet, D. Roderick, and Mathew D. McCubbins. 1991. *The Logic of Delegation: Congressional Parties and the Appropriations Process*. Chicago: University of Chicago Press.

Kiewiet, D. Roderick, and Kristin Szakaly. 1992. "The Efficacy of Constitutional Restrictions on Borrowing, Taxing, and Spending: An Analysis of State Bonded Indebtedness, 1961–1990." Presented at the annual meeting of the American Political Science Association, Chicago.

Kincaid, John, and Joel Lieske. 1991. "Political Subcultures of the American States: State of the Art and Agenda for Research." Presented at the annual meeting of the American Political Science Association, Washington.

Lieske, Joel. 1993. "Regional Subcultures of the United States." *Journal of Politics* 55:888–913.

Lowery, David. 1985. "The Keynesian and Political Determinants of Unbalanced Budgets: U.S. Fiscal Policy from Eisenhower to Reagan." *American Journal of Political Science* 29:428–60.

Luttbeg, Norman R. 1971. "Classifying the American States: An Empirical Attempt to Identify Variations." *Midwest Journal of Political Science* 15:703–21.

McCubbins, Mathew D. 1991. "Party Governance and U.S.

Budget Deficits: Divided Government and Fiscal Stalemate." In *Politics and Economics in the Eighties*, ed. Alberto Alesina and Geoffrey Carliner. Chicago: University of Chicago Press.

Morgan, David R., and Robert E. England. 1987. "Classifying the American States: An Update." *Social Science Quarterly* 68:405–17.

Musgrave, Richard. 1959. *The Theory of Public Finance*. New York: McGraw-Hill.

Nice, David C. 1991. "The Impact of State Policies to Limit Debt Financing." *Publius* 21:69–82.

Nordhaus, William D. 1975. "The Political Business Cycle." *Review of Economic Studies* 42:169–90.

Poterba, James M. 1986. "Explaining the Yield Spread between Taxable and Tax-exempt Bonds: The Role of Expected Tax Policy." In *Studies in State and Local Public Finance*, ed. Harvey S. Rosen. Chicago: University of Chicago Press.

Poterba, James M. 1989. "Tax Reform and the Market for Tax-exempt Debt." *Regional Science and Urban Economics* 19:537–62.

Poterba, James M. 1994. "State Responses to Fiscal Crisis: The Effects of Budgetary Institutions and Politics." *Journal of Political Economy* 102:799–821.

Regens, James L., and Thomas P. Lauth, Jr. 1992. "Buy Now, Pay Later: Trends in State Indebtedness, 1950–1989." *Public Administration Review* 52:146–56.

Roubini, Nouriel, and Jeffrey D. Sachs. 1989. "Political and Economic Determinants of Budget Deficits in the Industrial Democracies." *European Economic Review* 33:903–38.

Rowley, Charles K. 1987. "The Legacy of Keynes: From the General Theory to Generalized Budget Deficits." In *Deficits*, ed. James M. Buchanan, Charles K. Rowley, and Robert D. Tollison. New York: Basil Blackwell.

Schick, Allen. 1993. "Governments Versus Budget Deficits." In *Do Institutions Matter?*, ed. R. Kent Weaver and Bert A. Rockman. Washington: Brookings.

Schwarz, Gabriel. 1978. "Estimating the Dimension of a Model." *Annals of Statistics* 6:461–64.

Stein, Robert M. 1984. "Municipal Public Employment: An Examination of Intergovernmental Influences." *American Journal of Political Science* 28:636–53.

Stock, J. 1987. "Asymptotic Properties of Least Squares Estimators of Cointegrating Vectors." *Econometrica* 55:1035–56.

Tullock, Gordon. 1987. "The General Irrelevance of the General Theory?" In *Deficits*, ed. James M. Buchanan, Charles K. Rowley, and Robert D. Tollison. New York: Basil Blackwell.

Wright, Gerald C., Robert S. Erikson, and John P. McIver. 1985. "Measuring State Partisanship and Ideology with Survey Data." *Journal of Politics* 47:469–89.

Zimmerman, Dennis. 1991. *The Private Use of Tax-exempt Bonds: Controlling Public Subsidy of Private Activity*. Washington: Urban Institute Press.

James C. Clingermayer is Assistant Professor of Political Science, University of Texas at El Paso, El Paso, TX 79968.

B. Dan Wood is Associate Professor of Political Science, Texas A & M University, College Station, TX 77843-4348.

American Political Science Review Vol. 89, No. 1 March 1995

AVARICE AND AMBITION IN CONGRESS: REPRESENTATIVES' DECISIONS TO RUN OR RETIRE FROM THE U.S. HOUSE

RICHARD L. HALL *University of Michigan*
ROBERT P. VAN HOUWELING *Office of Senator Thomas A. Daschle*

Over two centuries ago, Adam Smith wrote of two passions that motivate those seeking public distinction: avarice and ambition. By investing these categories with appropriately concrete meaning, we develop a model of House members' career decisions. Like other individuals contemplating retirement options, politicians act with an eye to their financial interests, but not all financial interests are alike. The financial factor that matters most involves perennial considerations of post-retirement pension benefits, not fleeting opportunities to exploit ethically questionable sources of outside income. Second, we embed in the model a theory of intra-institutional ambition. Members impute value both to leadership positions they expect to retain and positions they expect to obtain. Majority members well-positioned to exert future legislative leverage are less likely to retire. Finally, several sources of electoral insecurity increase retirement probability. In the main, members may be reelection-seekers but will not pay any price to seek something they may not find.

Over the last two decades, questions about the causes and consequences of turnover in the U.S. House have grown increasingly salient among students of legislative elections and institutional change. By the beginning of the 1990s, concern over the apparent invincibility of House incumbents grew evident in the broader public as well. Proposals to impose term limitations on House members became less the faint cry of the reformist fringe and more the popular cause of citizens in several states. Intransigent budget and trade deficits, political stalemates on major policy issues, and sharp drops in the institution's already low approval ratings only helped fuel such causes.

Whither the House incumbent in this context? "Should I run again?" became for many members an increasingly problematic matter, such that a number of high-profile individuals decided to opt out of Congress. How might we explain such behavior? Why do single-minded seekers of reelection sometimes *not* seek reelection?

In addressing this puzzle, we begin with what Adam Smith would refer to as a politician's "moral sentiments"—the relative balance of "avarice and ambition" in the "passions" of those who seek distinction (1801, 1:98–99). Or if one prefers the language of a latter-day sociologist, Max Weber, to that of the eighteenth-century father of economics, we explore the extent to which the House member's vocation is one of living "off politics" or living "for politics" (1965, 9). Once invested with appropriately concrete meaning, we believe these categories hold the key to understanding the career decisions of professional legislators.

Our first line of inquiry focuses, then, on the "avaricious" interests of legislative incumbents. Less pejoratively put, we model the different financial incentives that differently situated members have to remain or retire from office. Members of Congress, we assume, are not expected to foreswear *all* considerations of financial self-interest, despite the label "public servants." In making such career decisions, however, we argue that members weigh most heavily legitimate factors affecting their postretirement income stream, rather than passing opportunities to exploit ethically questionable windfalls.

Second, we elaborate and systematically test a theory of *intra-institutional* ambition—what Weber might have referred to as living "for politics." To the well-worn assumption about single-minded reelection seeking, we would simply add that members who seek reelection do so with a postelection purpose. In general form, the argument is that House members are ambitious *within* the House. More specifically, their calculations about the value of running for reelection turn on the expected policymaking value of the positions they are likely to hold in the *next* Congress. Previous studies have examined the importance of members' institutional positions. But to date these investigations have not been embedded in an explicit theory of legislative organization, from which one might derive expectations regarding the structural opportunities of individual members for upward mobility. We attempt to do that.

Finally, we investigate the influence of electoral vulnerability on members' electoral ambition. In so doing, we extend the literature on strategy and choice among candidates for office by calling attention to the factors affecting the expected personal and psychological costs associated with mounting a potentially unsuccessful reelection campaign. Members do *tend* to be reelection seekers; they exhibit "static ambition," to use Schlesinger's (1966) awkward phrase. But seeking reelection is not cost-free. And in the minds of members, in any case, a happy outcome is never certain (e.g., Kingdon 1968; Mann 1978).

In sum, we advance a behavioral theory that characterizes members as financially self-interested, institutionally ambitious, and electorally insecure. We test the theory using data from the 1991–92 electoral cycle—a period especially well suited for achieving analytical leverage on the several hypotheses we advance regarding member decisions to seek reelection or retire from office. Over 50 members of the House chose not seek elective office in 1992, setting a postwar record and posing both a challenge and an opportunity for theorists of individual choice and legislative turnover.

WHY DO MEMBERS RETIRE?

We are hardly the first to pose this question. In particular, a large literature has investigated patterns of aggregate turnover across Congresses (e.g., Cooper and West 1981; Hibbing 1982; Polsby 1968), from which scholars have attempted to draw inferences about the motivations of retirees. Recent work, in turn, has begun to model the individual-level choice process, the phenomenon we must ultimately understand to fully comprehend the changing patterns we observe in the time series of biennial Congresses. Kiewiet and Zeng (1993) provide the first and most ambitious attempt in this regard, developing an expected utility model of House members' career choices and testing it with data for the period 1947–86. Similarly, Groseclose and Krehbiel (1994) provide a decision-theoretic model of House members' retirement decisions, albeit they focus (as we do) on the cross-sectional variation in member decisions to run or retire in a single election cycle.

Both of these studies provide important advances in developing a theoretical framework that stands to illuminate perennial questions regarding career choices and legislative turnover. We extend their work here, beginning with the premise that such decisions are best understood against the backdrop of a general model of rational choice. But the principal theoretical payoff, we believe, lies less in the general assumptions one invokes than in the substantive meaning of the question, "Rational and strategic with respect to what?" What does one pour into the abstract vessel of utility in order to understand the particular decisions of legislators in the world?

In answering this question, our study differs significantly from previous work. Unlike Kiewiet and Zeng, for instance, we develop a theory that incorporates financial considerations into the choice process, a factor that dominates research on career change and retirement decisions in the large literatures on private and public labor economics. Unlike Groseclose and Krehbiel, we focus on financial factors that perennially arise, not fleeting features of a single cycle. And unlike both works, we locate the expected utility calculus of the member in the context of a larger theory of intrainstitutional ambition.

Personal Income in and beyond the House

It is perhaps tautologically true that members of Congress, unlike most of us, have political ambitions. At the same time, most members, like most of us, have private lives and personal interests about which they worry. Financial self-interest, we have little reason to believe, entirely disappears through the several processes of recruitment, selection, and political socialization that precede individuals' ascension to elective office. In this generic sense, then, we assume that House members are "avaricious."

Among the general public, various good-government groups, and cynical wits of long standing, this last point would undoubtedly count as a gross understatement. "To my mind," Mark Twain (1873) observed, "Judas Iscariot was nothing but a low, mean, premature Congressman." During the early 1990s, the financially self-interested nature of legislators became an especially pervasive theme in the esteemed tradition of Congress bashing. More important for our purposes, financial self-interest took several different manifestations during this period, making it an analytically valuable focus to investigate avarice in member career decisions.

Without doubt, the matter that generated the greatest public disdain was the pay raise that House members approved for themselves as part of the Government Pay and Ethics Act passed in late 1989. Under this bill, House members' salaries would jump over 40% over three years, an action that sparked cries of public outrage as loud as the House had heard in decades. At the same time, advocates argued that long overdue pay increases were necessary to recruit and *retain* good members (e.g., Ornstein 1989), a simple point that echoed well-established themes in labor economics. The level of compensation in Congress, as in other public or private organizations, should affect the probability that members will remain.

Unlike most congressional pay raises, however, the annual financial impact of the increase was not constant across members. The raise was part of a compromise *package*, which (among other reforms) included a simultaneous ban on the much-maligned income that many members received from honoraria[1]—direct payments from interest groups to representatives in return for some nominal service, typically a short speech or public appearance. The personal income that members received from such honoraria ranged from zero to the legally imposed cap (approximately $27,000 per year for 1989–90), with considerable variation in between. To the extent that a member supplemented his or her congressional salary in this way, the pay raise would thus be offset by the opportunity cost of the honoraria ban. Indeed, for those hitting the legal honoraria limit, the pay increase and the honoraria ban was almost a perfect wash, the authors of the package having designed it deliberately so. The first effect of the pay raise package that Adam Smith might hypothesize, then, is that the probability of remaining will be greater for those who anticipate a real and nontrivial increase in congressional income. For many, that simply was not the case.

A second issue that fueled the critical flames in the early 1990s turned on the fact that under existing

Federal Elections Campaign law, members who were serving in Congress before 1980 were eligible upon retirement to convert leftover campaign funds to personal use. Such members were Federal Election Campaign Administration (FECA) "grandfathers." However, the same law that raised members' pay and banned honoraria also banned any conversions after 1992. The resulting financial incentive to retire by 1992 was thus the subject of considerable speculation. Might this last-chance opportunity to convert campaign funds to personal gain incline grandfathered members to retire when they might otherwise run? Or (to use a metaphor common at the time) might they jump from atop their Washington careers with the aid of a "golden parachute" (e.g., Chaffee 1991). Groseclose and Krehbiel recently investigated this hypothesis in systematic fashion, concluding that the cynical view was true:

> By a substantial margin golden parachutes were the main cause of retirements from the 102nd Congress. According to [our estimates], the not-so-subtle financial inducement that was carefully placed in the FECA amendments caused nearly twice as many retirements as redistricting and nearly four times as many retirements as the House Bank scandal. . . . While popular accounts and this account of the 1992 election differ in specifics, their similar and more general implications are compatible with what seems to be a growing cynicism directed toward the contemporary Congress. [Our] findings underscore the high likelihood that many 1992 retirees were, in effect, bought out of office. (1994, 95)

While both the golden parachute and *honoraria lost* hypotheses strike us as plausible, we here advance an alternate, perhaps less pejorative, and (more importantly) perennially applicable account of "avarice" in members' career decisions. The financial factors that we expect to matter most do derive from the pay raise package, but they involve calculations regarding changes in the long-term stream of postretirement pension benefits, not financial windfalls of the 1992 moment. That package, like a dozen others in the postwar period raised House members' annual salaries ("Congress Hikes Pay" 1989, 60). By preexisting (and hence far less visible) law, however, any given pay raise automatically increases the present value of members' pension annuities. The pension implications of a raise, moreover, are highly variable across members, given their differing years of service, age, life expectancy, and the like. Incorporating such implications into a model of the decision to run or retire, we believe, is crucial. Simply put, one cannot theorize about the financial incentives of individuals to retire in any given year without (1) placing the choice in a framework that reflects the strategic foresight that rational members would have employed during the prior cycle (i.e., 1990), and (2) reflects the analogous foresight a member should exhibit in the current cycle (i.e., 1992) regarding the implications of postponing the retirement decision yet one cycle hence (i.e., 1994).

Such a framework brings the pensions, not the honoraria or campaign cash, into sharp relief, in that the latter two constitute financial incentives to retire that were at least as large in 1990, when voluntary retirements totaled seven, as in 1992, when they totaled 53, and did not exist for 1994 and subsequent cycles. For two additional reasons, we hypothesize that members' calculations regarding expected changes in their pension benefits should dominate other financial incentives in member decisions to run or retire in 1992. One is that through both public debate and final passage of the 1989 reform package, members of the House collectively acknowledged that the first two—leftover campaign cash and honoraria—were ethically suspect sources of member income. To keep either would likely incur serious reputational costs for a member who planned to be anything but a postretirement recluse, not to mention evoking the member's ethical sensibilities more keenly.

The second and more important reason owes to the sheer magnitude of the expected benefits implied by the pension increases. While the particulars of the pension formula are described in the Appendix, it is sufficient to note here that annual pension benefits are calculated based on a member's three highest annual salaries. Members retiring in 1990 would not simply have forgone the short-term remunerative benefits of the forthcoming pay increases. More importantly, they would have forgone the dramatic increase in the present value of the pension benefits that would have accrued by delaying retirement two more years.

Similarly, members who contemplated retirement in 1992 should also have anticipated the financial implications of retiring in 1992 versus 1994. Partly as a result of the 1989 pay raise package, there would be yet another (albeit smaller) increase in the present value of a member's pension over the period 1993–94, as the moving average of the member's three highest salary years would continue to increase. In principle, then, pension-minded foresight with respect to 1994 might give the prospective 1992 retiree an incentive to remain, not retire. Greater uncertainty necessarily enters such longer-run calculations, however. Indeed, while substantial pension benefits from the 1989 package were already in the bank, members had good reason to doubt that continued increases in their annuities would materialize at all, given the widely discussed prospect that the 103d Congress (1993–94) might *cut* congressional pay and diminish the generosity of the pension formula.[2]

Statistics reported in Table 1 capture in aggregate form the financial implications of the 1989 package over the period 1990–94, with all variables measured in present value terms (see Appendix). The first column shows that the yearly salary benefit fully materialized only if one remained rather than retired in 1992. The simple statistics on honoraria income suggest, in turn, that the honoraria-minded member had, if anything, a stronger incentive to retire relative to his colleagues in 1990 than 1992.

More strikingly, Table 1 reveals that the changing value of the typical member's pension annuity swamps the potential campaign cash windfall and, to

TABLE 1

Financial Considerations of Potential 1992 House Retirees: 1990–1994

		FINANCIAL VARIABLES					
		HONORARIA INCOME ($)		CONVERTIBLE CAMPAIGN CASH ($)		MEDIAN MEMBER PENSION BENEFITS[a]	
YEAR	ANNUAL SALARY (CURRENT $)	MEAN	MEDIAN	MEAN	MEDIAN	PRESENT VALUE	% INCREASE
1990	96,600	10,011	4,450	94,700	0	412,258	26.6
1992	129,500	0	0	85,974	0	597,689	45.0
1994	133,600	0	0	0	0	738,198	23.5

Note: N = 422.
Sources: Federal Elections Commission, "For the Record" 1990, 1750–53; U.S., Bureau of the Census 1992.
[a] A median member is defined here as a white male, age 55 in 1990, with 10 years in the House, values which closely approximate the medians of the variables most relevant to calculating the present value of the pension increases (see Appendix).

an even greater degree, the income lost by the honoraria ban. By delaying retirement from 1990 to 1992, the typical member would increase the present value of his or her pension by $185,000, a 45% jump!

Of course, the medians do not tell the whole story. A minority of members stood to convert substantial sums of campaign funds to their personal use (thereby pulling the campaign cash mean well away from the median). But note that this same opportunity existed if they retired in 1990 (and years prior as well), whereas the substantial increase in one's pension benefits required that the member remain in office until 1992. The 1989 pay raise legislation, in short, should have produced a substantial, pension-driven propensity to delay retirement in 1990. That propensity, in turn, should materialize in the decisions of individuals to retire in 1992. Given the much less certain prospect that pension annuities would continue to increase after 1992, however, there is less reason to predict that delayed retirements would carry over into the 1994 election cycle.

Progressive Ambition *within* the House

Two hundred years ago, an American constitutional theorist, John Adams, reflected on Smith's *Theory of Moral Sentiments* as he investigated what he took to be the metaphysics of republican government. Adams wrote in *Discourse on Davila* of man's "passion for distinction" which drove many members of the "natural aristocracy" to public life. Once in office, Adams elaborated, this passion did not diminish: "When it aims at power," Adams observed, this passion "is called *Ambition*" (1954, 176–77; emphasis original); and Adams, like Madison, saw the ambitious nature of men in government as a fundamental assumption upon which good governmental institutions should be built.

Studies of ambition *within* Congress are considerably more rare among contemporary political scientists. Probably the most important exception is Fenno's oft-cited finding that representatives harbor both the desire to make "good public policy" and the desire for "power within the House"—motives different from, and at least partly independent of, their desire to win reelection or realize financial gain (1973, ch. 1; see also Loomis 1988).

The point here is not to distinguish between members' extraelectoral motives, but to emphasize that members who seek reelection do so with some postelection purpose. In short, the assumption that one runs simply to run again (two years hence) is suitable only for a behavioral theory of pet gerbils on a cage-wheel. Rather, our point of departure is that lest ambition be somehow purged from a legislator's political heart, the expected value of his or her position in terms of its intrachamber benefits will affect the decision to run again or retire from office. Legislators, like other professionals, seek and assess the opportunities for upward mobility as they go about calculating the value of the position they hold within their organization. Ascending to the seat of, say, committee chair or deputy whip may be mostly imperceptible to the people of Poughkeepsie, but such positions do mark distinction in the vicinity of the Potomac. Moreover, these positions count as distinctions with a legislative difference, in that they typically bestow certain procedural prerogatives, seats at important bargaining tables, special access to relevant political and policy networks, and other sources of legislative advantage.

In the U.S. Congress, there are many positions of relative power in many different policy domains. As Kenneth Shepsle (1978) has emphasized, the structure of such opportunities within Congress is governed for most members by the nature of the committee system and the processes that lead to assignment and ascension within it. A select few, for instance, hold the coveted position of a committee chair. These positions, in particular, bestow most of the institutional advantages just catalogued. To a smaller but still substantial degree, the same holds for representatives who become chairs of House subcommittees. These individuals typically enjoy considerable agenda power within their more specialized domains, as well as a budget and staff to subsidize the pursuit of their own legislative objectives. Full committee and subcommittee ranking minority members also enjoy some legislative advantages. Most receive some subsidies in the form of additional staff or budget, as well as access to important political networks. In short, holding a committee leadership position holds institutional value, but that value should prove variable across positions both within and across majority and minority party. Given that House members could, with high probability, expect that they would do no worse than retain the positions of value currently held, such positions ought to incline members to run rather than retire.

But what of committee backbenchers? They are ambitious, too, we assume. Moreover, their ambitious are less fully realized. To import the language of Schlesinger from the electoral to the legislative realm, they must be "progressively ambitious" if ambitious they be. The key point to be made here is that the *expected* value of the committee backbencher's current seat is not reducible to the *current* value of his or her position, as one might reasonably assume, say, for full committee leaders. The basis for this claim derives from the theory of legislative organization elaborated by Shepsle (1978). To the typical member, Shepsle argues, committees appear as opportunity "queues," and different members have different subjective estimates of the value of different queues. Based in part on those estimates, members seek initial assignment (or subsequent transfer) to some committees and not others. Party leaders endowed with the authority to make assignments, in turn, act so as to accommodate member requests as much as possible. Once on a committee, then, the strong (albeit not inviolable) seniority norm sustains expectations that members will, with time and turnover, move up the queue toward a formal position of power.

The implication for a theory of voluntary retirements, then, is that backbenchers will engage in strategic foresight with respect to the development of their in-House careers. More specifically, member estimates of the expected value of remaining turn not simply on what position the member currently holds but also on calculations regarding how far and how quickly the member might advance in the specific institutional queue(s) in which he or she sits.[3] Our account of intrainstitutional ambition thus implies that past treatments of the "value of a seat" have been theoretically uninformed. By themselves, dichotomous indicators of current position, especially when collapsed into an undifferentiated "leadership" variable, do not capture the expected value of a member's seat. In particular, they provide not at all for ambitious members' rational expectations regarding upward mobility in the House organizational structure.

While committee queues are important in a decentralized assembly, however, they are not the only routes by which members might realize upward career mobility within their chamber. While far less important in Congress than in parliamentary systems, the respective party organizations within one's chamber likewise include positions of distinction and political importance. Members can pursue upward mobility within these organizations and perhaps profit thereby; indeed, some members attempt to do so while simultaneously pursuing advancement within the substantive committees on which they sit.

The processes of advancement within one's party leadership organization, however, are more complicated and less predictable than those within committee. The lower positions (e.g., regional whips) hold little current value and incur nontrivial costs in terms of time allocated to doing the leadership's bidding. Ascension to higher positions, in turn, is less a function of time, turnover, and advancing seniority than it is party loyalty and effort in advancing the party leadership's agenda. Though this be a path of intrainstitutional advancement that some members take, then, the time required over multiple terms and the greater uncertainty surrounding the ultimate ascension to important positions make the expected value calculations of such individuals simply difficult to estimate (for them and us). The best we can do is to identify individuals with official rank in their party's organization and assume that having one rank is a precondition for achieving a higher rank, which in turn might incline a member to run rather than retire. Our expectation here, as above, will be that a place in the majority leadership ranks will hold greater value than a place in the minority's party organization.

Electoral Ambition and Its Discontents in 1992

The financial and intrachamber career incentives to run or retire having now been elaborated, we are still left with a partly empty theoretical glass. Most members were not last-chance FECA grandfathers; others were multiple-terms away from realizing significant pension benefits; still others held significant institutional positions. Yet many in each category retired.

As others have noted, electoral considerations invariably come into play. Members who see themselves as vulnerable will be less inclined to pay the personal and psychological costs of mounting a campaign than members who feel politically more secure. What factors are likely to affect these subjective estimates of electoral vulnerability? Kiewiet and Zeng (1993) emphasize the importance of public scandal—a perennial problem but one that typically besets only a few members in any given election cycle. In 1992, however, the wind of scandal blew across numerous members in varying intensities (Groseclose and Krehbiel 1994). During the 102d Congress, information became increasingly public that scores of named House members had repeatedly overdrawn their checking accounts at the House bank. While no public money was at risk, the "check-kiting scandal" sullied the reputation of those members who had overdrawn their accounts, especially those who "distinguished" themselves by writing rubber checks in the hundreds. The check-kiting debacle, of course, was specific to 1992, and given the ensuing reforms of the House Bank, nothing quite like it is likely to recur. But its breadth and variability across members provides a good opportunity to assess the more general effects of reputational damage, which in turn should affect the psychological costs of facing the voters and the greater risk, in the end, of losing their favor.

Other factors likely to affect a member's perception of electoral vulnerability are even less specific to 1992 incumbents and have also received considerable attention elsewhere. One is reapportionment. Every 10 years district boundaries are redrawn, and in the process some incumbents invariably suffer significant damage to their electoral base. In some cases, in fact, the resulting district lines produce races that pit incumbent versus incumbent, but the threat to a member's electoral security is typically less dramatic. The more general claim is that members will feel more threatened to the extent that reapportionment altered their district boundaries in a way that placed politically less compatible constituents in their district.

Finally, the reelection-related factor most commonly examined in the several literatures on electoral competitiveness, retirements, and representation is the magnitude of a member's previous electoral margin. Indeed, scholars have invariably used this variable to define the extent to which particular seats are "marginal" or "safe," and members themselves use the statistic to help them assess how much political trouble they might be in. Otherwise successful politicians, we assume, do not relish public rejection. They would rather "voluntarily" retire to private life than suffer the public pain of involuntary defeat.

Other Factors: Rounding Up Some Usual Suspects

Perhaps the most intuitively obvious factor in any account of (public or private) retirements is the potential retiree's age. The point here is not that there is either a rule or an informal norm within Congress about the appropriate age to retire. Neither is it the case that advancing age captures well House members' opportunities to substitute more rest and relaxation for hard labor in a job they do not necessarily like (as one might assume for most private citizens). Members of Congress are a small, largely self-selected pool who pay great costs to get and stay there and most of whom, in any case, are eligible for significant pension benefits well before age 62, the first threshold for Social Security and many private retirement plans. Rather, age is not so much the phenomenon of interest as it is a rough proxy for the health and physical wherewithal necessary to meet the demands of the job.

The final set of frequently cited causes for retirement are those related to increasing disaffection with service in the House. This factor has been emphasized in the popular press, replete with quotations from retiring members who cite increasing frustration with legislative gridlock, budget constraints, and various extra legislative aspects of service. Descriptions of their own reasons notwithstanding, we doubt that the disillusionment with the institution so frequently described in members' retirement retrospectives is behaviorally significant (Hibbing 1982). We do not so much consider members' public statements insincere, however, as see it in the context of the larger theory of intrainstitutional ambition we have elaborated. Again, the premise here is that members serve with a postelection purpose, some purpose independent of achieving yet another reelection.

That purpose is especially elusive for those who are and expect to remain in the minority. With time and turnover, the principal positions of power will still elude them. Prior to 1994, for instance, Republican claims of disaffection became far more frequent and intense than those of Democrats (Cheney 1989; Foerstel 1992). Prominent Republican retirees increasingly complained that they were locked into minority status, that the standing rules of the House were unfair to the opposition, and that the majority party leadership deployed special rules to circumvent the minority's parliamentary rights.

But how is this language of disaffection to be interpreted? The political scientist might lament that there are not (nor are there likely ever to be) valid data on members' relative levels of "disaffection." But our point is a different one, namely, that this notion has not to date been placed in a conceptual frame that would render it interpretable. We do so here. The rhetoric is consistent with the assumptions about ambition central to our account. Talk of "disaffection," we hypothesize, reflects the presence of dashed or diminished expectations about the minority's opportunities for intrainstitutional goal fulfillment. As one minority member lamented, "There is a lack of an opportunity to make a contribution." Or, as the ranking member of one major House committee put it, "I can't do anything but nibble at the edges."

DATA AND MEASUREMENT

To test the theory of choice elaborated in the preceding section, we estimate a dichotomous probit model of member decisions to run or retire in 1992—a year particularly appropriate for our purposes in that it enables us to test the general claims we advance against some of the more transitory factors that have captured the attention of both scholars and Congress-watchers. The dependent variable is 1 if a House member retired from elective office in 1992, 0 if he or she ran for reelection. We thus exclude from our analysis the 13 members who retired from the House to seek higher office.[4]

Financial Incentives. Proper measurement of individual members' financial incentives to run or retire is a complicated matter and, given the important implications one might draw from the ensuing results, must be handled with special care. Recall that a House member's financial interests for the period of our analysis are not all of a piece. There is the opportunity for some members to convert personal campaign cash to their own use: the golden parachute windfall. There is the differential effect of the pay raise package on members' annual income, given the elimination of the variously exploited provision that members could accept as much as $27,000 in interest group honoraria. And there is the increase in the value of members' pensions, likewise a result of the 1989 pay raise package.

To quantify such factors so that we might make reasonable comparisons among them requires converting them to a common scale, namely, their 1992 present value. The Appendix provides the relevant formula, as well as other particulars of measurement.

Ambition and the Expected Value of the Seat. For reasons we have articulated, measurement of the expected value of a member's seat if elected requires several terms. First is the value of the seat that a member can reasonably expect to retain, namely, the member's committee, subcommittee, or party leadership position(s). Since the expectations of minority members regarding the realization of their policy goals should be lower, we include separate terms for the leadership positions within each party, as well as a dichotomous term for majority party membership.

Given our account of the variable opportunities for upward committee mobility, the ideal strategy for capturing members' progressive ambition would be first to place each in each of their relevant institutional queues. One might then produce an expected value estimate as a function of the number of persons ahead of each member and the joint probability of outlasting all of them. As a first cut at this problem, we deploy a considerably simpler test. We assume that in making run/retire decisions, members weigh most heavily their chances for significant career advancement in the forthcoming two-year Congress. This assumption reflects not simply the time value of one's career benefits, but also the greater uncertainty associated with longer time horizons. Looking beyond two or more biennial elections to estimate the probability of turnover for multiple members ahead in the relevant queue(s) (and one's own repeated reelection) is a highly unpredictable enterprise.

The individuals most clearly confronted with the prospect of upward mobility, then, are those who are second in a committee queue.[5] But as we have discussed, their expectations about advancement turn on the likelihood of outlasting the current leader. The measure we thus employ is the product of two terms: a dichotomous variable reflecting whether a member is second in a committee queue; and the age of the current leader minus the age of the second in line. (For instance, if a member is second in line yet older than the sitting chair, the value of this variable is, appropriately, negative.)[6]

Electoral Vulnerability. We measure marginality as the percentage point spread that the representative enjoyed in the top two candidate vote in the previous election or primary, whichever was lower. To capture the reputational damage generated by the check-kiting scandal, we use the number of overdrafts a member wrote at the House bank. Both variables are logged, on the assumption that they will have diminishing marginal effects in the mind of the electorally aware member.

Our final measurement task was to capture the member's subjective estimate of reapportionment-induced insecurity. We rely here on *Congressional Quarterly Weekly Report*'s coverage of redistricting. Using articles indexed under redistricting over the period January 1991 to the deadline for candidates' filings in 1992, we coded (0/1) whether *Congressional Quarterly* characterized redistricting as damaging each representative's electoral security—characterizations that included but were not limited to assessments of whether reapportionment was likely to pit incumbent versus incumbent. It is important to emphasize, however, that we excluded all articles about members' run/retire decisions, in that such reports sometimes included post hoc references to the reputed reasons for retirement (i.e., redistricting).

TO RUN OR NOT TO RUN: A DICHOTOMOUS PROBIT ANALYSIS

To summarize, we have developed an economic theory of House members' decisions to run or retire, a theory that decomposes the central incentives at work into what theorists from three continents across three centuries might refer to as "avarice and ambition." Table 2 presents the results of two separate probit analyses. The first represents the specification best fitted to our foregoing discussion in that it includes the 1990–92 but excludes the 1992–94 annuity increase. Recall that the latter exhibited greater uncertainty surrounding its ultimate payoff and, in any case, entailed a smaller incremental difference. Nonetheless, we report a second analysis that includes both variables so as to assess whether there might have remained a nonnegligible financial *dis*incentive to retire in 1992.

Taken together, the two sets of estimates are stable and provide solid evidence for several of our hypotheses. While the maximum likelihood coefficients must be translated into substantive effects (a matter we will take up momentarily), the coefficients, their signs, and levels of statistical significance are stable across equations and tell us several things. The first and intuitively least remarkable finding is that advancing age increases the probability of retirement in Congress, as in other organizations both public and private.

Second, the estimated effects of the several qualitatively different financial factors fit well with our predictions regarding representatives as (benignly) avaricious actors. As hypothesized, the critic's assertion that members had been "living off" congressional politics by taking interest-group honoraria finds little support in our analysis of their retirement

TABLE 2

Probit Analyses of House Member Decisions To Run or Retire, 1992

VARIABLE	MODEL 1	MODEL 2
Constant	−3.03 (.66)**	−3.08 (.73)**
Age	.023 (.011)**	.023 (.011)**
Financial incentives		
Honoraria kept (log)	.003 (.023)	.002 (.023)
Convertible campaign cash (log)	.021 (.018)	.021 (.018)
Pension increase 1990–92 (log)	.065 (.033)**	.060 (.037)*
Pension increase 1992–94 (log)	—	.008 (.035)
Institutional ambition		
Majority party leader	−.483 (.261)**	−.484 (.261)**
Committee chair	−.600 (.454)*	−.577 (.465)
Subcommittee chair	−.101 (.252)	−.102 (.251)
Majority party member	.230 (.328)	.233 (.329)
Minority party leader	.085 (.415)	.086 (.415)
Committee minority leader	.583 (.415)*	.587 (.415)*
Subcommittee minority leader	−.119 (.343)	−.120 (.343)
Age of committee chair − age of majority 2d in line	−.075 (.051)*	−.075 (.051)*
Age of committee minority leader − age of committee minority 2d in line	−.011 (.052)	−.011 (.052)
Electoral ambition		
Reapportionment damage	.734 (.213)**	.733 (.213)**
Number of overdrafts (log)	.099 (.047)**	.098 (.047)**
Winning margin 1990 (log)	−.222 (.114)**	−.225 (.114)**
Log likelihood	−125.70	−125.68

Notes: N = 422. Entries are maximum likelihood coefficients, with asymptotic standard errors in parentheses. *Winning margin* reports the lower margin of the primary or general election.
* $p < .10$.
** $p < .05$.

decisions. While correct in sign, the t-statistic on *honoraria lost* reflects essentially a null result.

More importantly, the widely asserted view that members floated into retirement on golden parachutes receives only modest support. While the sign of the coefficient is positive and nontrivial, one must adopt a generous statistical threshold to reject the null. Still, we do not conclude from Table 2 that the golden parachute hypothesis may be dismissed; to do so would risk committing a substantively important Type II error. Here and throughout we would simply emphasize that statistical confidence is a matter of degree, not of arbitrary threshold. Our more general objective is to assess the substantive significance of the behavioral effects, qualified by the degree to which they justify confidence in a statistical sense. Happily, subsidiary analysis is available to assess this hypothesis further.

By comparison, the probit results clearly suggest that the changing present value of members' pensions did affect their decisions to run or retire in 1992. As predicted, the coefficient on the 1990–92 pension increase is positive, with a confidence level near .025 in the first model. As we also hypothesized, model 2 reveals that the effect of the potentially large but highly uncertain post-1992 pension increase is slight. Indeed, the coefficient is incorrect in sign.

The multivariate results of Table 2 likewise offer considerable support for our theory of intrainstitutional ambition. All three of the variables that indicate positions of institutional advantage within the majority party—party leadership position, committee chair, and subcommittee chair—are in the predicted direction, with the first two statistically significant at .10 and .05 respectively.[7] Of greater theoretical interest, however, is the result regarding *progressive* ambition among well-positioned players in the majority party committee queue. Ceteris paribus, the younger a member second in line for a committee chair is relative to the chair ahead, the less likely the member will retire. Strategic foresight inclines the member to stay for the ambition he or she might yet fulfill, independent of what the member may have already realized.

In general, the majority/minority differences suggest that our emphasis on the respective opportunity structures faced by ambitious but differently situated members is well placed. A position in the majority party leadership inclines a member to stay; minority leadership position does not. Committee chairs are more likely to stay; their minority counterparts are more likely to retire. And majority members next in line for a committee chair are more likely to stay than their counterparts in line for a ranking minority position.

Finally, the factors associated with members' political vulnerability and hence the personal (albeit nonmonetary) costs of running another campaign clearly matter to their (re)electoral ambition. The presence of reputational damage as captured by the number of House bank overdrafts, the electoral vulnerability revealed by their margin in the last election cycle, and damage done to their electoral base by reapportionment—all increase the likelihood of retirement, and all are statistically significant at the .05 level. What Schlesinger originally referred to as "static ambition," our results show, often gives way to electoral realities in the expected utility calculations of elective politicians.

AVARICE AND AMBITION IN CONGRESS: THE SUBSTANTIVE EFFECTS

Tables 3–6 translate the probit coefficients of the first model into substantively more interpretable results. Each table presents the probability that a representative will retire if the variable being altered is set at the value indicated and all of the other independent variables are set at their mean.

Avarice in Congress: All That Glitters Is Not Gold (and Vice Versa)

Our two main theoretical concerns here go to the importance of avarice and ambition in members' decisions to run for reelection or retire from office. Avarice, of course, is a pejoratively loaded term in ordinary usage. As we have deployed the concept here, however, avarice refers simply to the weight that individual members attach to their financial self-interest, with the necessary qualification that avarice in Congress, as in everyday life, comes in different forms, perhaps in different ethical/reputational colors.

Prior to the 1989 reforms, the predominant concern of congressional critics was the direct flow of interest-group honoraria into members' personal pockets. In the aftermath of the honoraria ban, in turn, the critics focused on the campaign funds grandfather-clause of the 1989 package, deriding it as a belatedly addressed loophole that allowed many to retire in 1992 with the benefit of a legal but unethical severance bonus (Chaffee 1991; Renfro 1992). While it received much less media glare, finally, we have noted the effect of the package on members' pensions. What substantive impact *did* such financial factors have on members' retirement decisions?

Table 3 quantifies their effects. Here, as in ensuing tables, we use the mean and one standard deviation above and below the mean as the illustrative values. In the case of honoraria lost, we estimate an increase in retirement probability of a thin .4%. As we have argued, given that the typical House member's honoraria income was relatively small and its reputational costs nonzero, this result is not surprising. Alternatively, if the strong charges of Common Cause and other good-government groups (i.e., that many members used honoraria to "live off" Congress) were true, those members should have been relatively underwhelmed by the incentive to stay provided by the pay raise, their probability of retirement being thus greater. But such was not the case.

TABLE 3

Effects of Personal Financial Factors on the Probability of Retirement in 1992

VARIABLE NAME AND LOG VALUE	RETIREMENT PROBABILITY (%)
Honoraria kept	
1	8.0
6	8.2
10	8.4
Change low to high	+.4
Convertible campaign cash	
0	6.9
4	8.0
10	10.1
Change low to high	+3.2
Present value of pension increase 1990–92	
4	4.0
10	8.7
13	12.3
Change low to high	+8.3

Note: Values shown are the mean, minus, and plus-one standard deviation of the independent variable. Retirement probability estimates are calculated from model 1, table 2.

The greater attention in the 1991–92 cycle was paid to the potential turnover effects of the golden parachute loophole, if only because the sums of money at stake were in some cases huge. (Several members held over a million dollars in convertible campaign cash.) More relevant here, however, is the systematic analysis of Groseclose and Krehbiel, who concluded that "by a substantial margin, golden parachutes were the main cause of retirements from the 102nd Congress" (1994, 20; see also Ahuja et al. 1993). Table 3 suggests that this conclusion is simply incorrect. When placed in the context of a more general model of the run/retire decision, the data simply do not support a strong golden parachute effect. For instance, the probability of retirement for the individual with the minimum amount of gold (i.e., zero) is 6.9%. Compare this to those for whom the log of convertible campaign cash is one standard deviation above the mean and the probability of retirement increases only 3.2%. When we computed the retirement probabilities for the minimum and the maximum values of the golden parachute variable, in turn, the increase remained relatively modest at 4.6%.

Moreover, there are additional data and a subsidiary line of analysis that shed brighter light on the golden parachute hypothesis and the cynical view of representatives' ethical sensibilities that it represents. To this point, we have assumed that the available funds a member could convert to personal use captured the grandfathered member's financial incentive at the moment of the run/retire decision. Note here, however, that this implies an additional, important but, to date, unstated assumption that members who took the money and did not run would keep the money they ran away with. As we have already noted, however, converting such cash to personal use was widely regarded as ethically questionable, perhaps reputation-damaging, especially after 1989. This belief was widely articulated by members themselves during consideration of the reform package and collectively ratified by the bill's ultimate passage. Reflecting on the public statements of retiring FECA grandfathers, one close observer of the 1991–92 retirements noted that "most of the retirees said they would give the money away." Have they, in fact?

The answer is, simply, *yes*: for the most part they have. Table 4 summarizes FEC data on the campaign fund disbursements of 1992 retirees for the last quarter of 1991 through the 1993 end-of-the-year reports. The disbursements are coded into the general categories of conversion to personal use, contributions

TABLE 4

Running Away with the Campaign Funds? Disbursement Patterns of 1992 House Retirees ($)

VARIABLE	SUM	MEAN	MEDIAN
Campaign funds on hand as of 10/91	10,072,377	314,762	272,406
Disbursements through 12/93 by category			
Conversions to personal use	1,411,436	44,107	0
Operating expenses	719,240	22,476	19,042
Contributions to charities/ nonprofits	3,300,563	103,143	1,775
Contributions to political organizations	1,857,739	58,054	2,600
Refunds to contributors	65,997	2,062	0
Campaign funds yet to be disbursed through 6/93	2,641,186	82,537	0

Note: N = 32.
Sources: Federal Elections Commission, Candidate Campaign Committee semiannual and termination reports, Schedule B: Itemized Disbursements, 1991–93.

and refunds, and operating expenses.[8] As the table shows, one full year after the retirees had left office, the typical FEC grandfather had converted *no money at all* directly to his or her personal use. In fact, only 7 of the grandfathered 32 had taken any of the campaign money they allegedly took. Indeed, these 32 who took the money and didn't run turned around and didn't take 80% of it.[9] By far most of the money went to charitable organizations, nonprofit foundations, political parties, and campaign organizations of various sorts. Smaller amounts were reimbursed to contributors, both individuals and political action committees. In sum, not only was the golden parachute not the most significant factor in House members' 1992 retirement decisions, several kinds of evidence lead one to doubt that it had any significant effect at all. Differently put, the glitter of the parachute reflects relatively little behavioral gold.

The more general lesson here is that outside the context of a general model that specifies the full range of financial factors that might plausibly affect the decision to run or retire, one cannot reliably assess the statistical or substantive significance of any one of them. There was, in fact, a strong financial basis for retiring, but the ethically questionable golden parachute was not it. Rather, the main effect followed from the incentives of members otherwise inclined to retire in 1990 to delay doing so until the next cycle, given the substantial increase in the long-term stream of pension benefits generated by a substantial pay raise. Not only is the effect of the pension increase on retirements statistically significant, it's substantive impact on retirement is considerably greater, even when one assumes (incorrectly) pure larceny in the parachutist's heart. As Table 4 shows, when one uses comparable values to illustrate the behavioral difference, the pension factor increases the probability of retirement in 1992 from 4.0% to 12.3%—over 2½ times the change associated with convertible campaign cash and 20 times the change associated with honoraria lost.

Ambition within the House: Glory among the Majority

Table 5 shows the substantive effects of several variables that reflect intrainstitutional ambition. Both committee chair and majority party leadership status decrease the probability of retirement by more than 6%. The crude measurement of such positions' expected value notwithstanding, then, we detect evidence that majority members who prove (more or less) successful in their quest for power are more likely to find their current vocation worth the cost of trying to keep it.

The substantive effect of intracommittee, progressive ambition on the majority House member's decision to run or retire is considerably more striking. To the extent that a member, second in line in the committee majority queue, is younger than the committee chair, he or she is much more likely to remain in the House. Table 5 reports the probabilities of retirement for members second in a committee queue who are five years older than, the same age as, or five years younger than the current chair. The chances of the chair-in-waiting who is five years *older* of outlasting his or her committee superior and thereby ascending to chair status appear small; the probability of his or her retiring is a substantial 18.0%. For the chair-in-waiting who is the same age as the current chair, however, the probability of retirement drops to 8.5%. And for those in the enviable position of being five years younger that the sitting chair, the probability of retirement drops again by half, to 3.4%—a change of 14.6% from the relatively old to the relatively young member second in the queue. Neither do these values capture the full potential of this variable's effect. For instance, when we generate the retirement probabilities associated with the minimum and maximum values on this variable (as we did for, e.g., the golden parachute hypothesis), the retirement probability plummets from 88.7% (28 years older than the chair) to .02% (24 years younger)![10]

Despite the somewhat exploratory nature of this analysis, in short, we find strong evidence that majority members exhibit strategic foresight in calculating the expected benefits of continued service in their chamber. The opportunities individuals perceive for upward mobility, in Congress as in other organizations, exert powerful effects on their career decisions.

Similar results do not materialize for members of the House minority. As our theory of progressive ambition led us to predict, minority members should

TABLE 5

Institutional Ambition and House Retirements in 1992

VARIABLE NAME AND VALUE	RETIREMENT PROBABILITY (%)
Majority party position	
Member – majority party leadership	
0	10.0
1	3.9
Cumulative change	−6.1
Committee chair	
0	8.6
1	2.5
Cumulative change	−6.1
Minority party position	
Member – minority party leadership	
0	8.1
1	9.5
Cumulative change	+1.4
Committee RMM	
0	7.8
1	20.1
Cumulative change	+12.3
Progressive ambition	
Age of committee chair – age of second in majority queue	
−5	18.0
0	8.5
+5	3.4
Cumulative change	−14.6
Age of committee minority leader – age of second in minority queue	
−5	9.0
0	8.2
+5	7.4
Cumulative change	−1.6

Note: Values shown are the mean, minus, and plus-one standard deviation of the independent variable. Retirement probability estimates are calculated from model 1, table 2.

be considerably less likely to anticipate positions of distinction and legislative advantage equal to their majority counterparts. The opportunities to affect policy being considerably less, the lure of intra-House ambition should thereby be less and the language of disaffection more often on the minority's lips. Even so, holding (or nearing) a minority leadership position should be preferable to minority backbencher status, such that the minority ambition variables should diminish the probability of retirement, at least slightly. Instead, we find that certain minority positions decrease while others increase the probability of retirement. For the more junior positions—subcommittee ranking minority members and those well positioned to assume ranking full committee status—the effects are predictably negative, albeit slight. For those who have already attained minority leadership or committee ranking status, however, the probability of retiring in 1992 was surprisingly higher. Indeed, being a committee ranking minority member increased the probability of retiring over 12%. Hence the paradoxical result: while minority members about to attain a full committee leadership position appear (slightly) more inclined to remain, those already there are more inclined to retire. Perhaps the latter simply know too well how little there is in store for the minority member with policy ambitions.

Rational Fear, the Pain of a Campaign, and the Decision to Retire

As others have suggested, the calculations of incumbents regarding their electoral vulnerability should figure prominently in the run/retire decision (Groseclose and Krehbiel 1994; Kiewiet and Zeng 1993). Ceteris paribus, members value reelection. At the same time, various factors affect members' estimates of the time, energy, and psychological costs of a reelection campaign.

What impact, then, do the factors associated with electoral vulnerability have on the incumbent's decision to run or retire? Table 6 quantifies the effects in the same manner as the prior tables. Especially noteworthy are the adverse effects of reapportionment on a member's constituency base and the ostensible scandal of overdrawing one's House checking account, findings that are consistent with other recent studies of member career decisions (e.g., Groseclose and Krehbiel 1994; Kiewiet and Zeng 1993). Where redistricting plans were reported early in the process to be damaging to the incumbent, the member's probability of retirement from public office more than tripled, from under 7% to over 22%.

Having bounced a check or two did not by itself evoke strong campaign-related fear in the incumbent's heart. We calculated that the retirement probability of the member who bounced 50 checks was virtually indistinguishable from the member who bounced none at all. Nevertheless, those who kited high enough to distinguish themselves (one standard deviation more reputationally deviant than the mean) exhibited a retirement probability over twice as large (12.1%) as the individual unsullied by the House Bank scandal (5.9%). As for the very worst abusers (who thus received the most and the worst publicity), their electoral anxiety rate exhibited a significant quickening. To embellish the illustrations of Table 6, members whose overdrafts approached the extremes (600+, of whom there were eight), the probability of retirement jumped to over 18%.

Finally, members' margin of victory in the last election also affected members' strategic electoral calculations. The retirement probability of a member whose (logged) margin was one standard deviation below the mean was six points higher than one who had a margin one standard deviation above. Two points about this finding are worth emphasizing, however. First, a significant number of members exhibited 1990 margins above minimum winning at

TABLE 6

The Effects of Electoral Fear on Electoral Ambition in 1992

VARIABLE NAME AND VALUE	RETIREMENT PROBABILITY (%)
Reapportionment damage	
0	6.6
1	22.1
	+15.4
Overdrafts – logged value	
0	5.9
2	8.7
4	12.1
	+6.2
Winning margin: 1990 election cycle – logged value	
2	10.8
3	7.3
4	4.8
	+6.0

Note: Values shown are the mean, minus, and plus-one standard deviation of the independent variable. Retirement probability estimates are calculated from model 1, table 2.

or near the extremes (i.e., 0, 50). When one compares the retirement probabilities associated with the log of these two values, the former (21.4%) is over four times the latter (4.8%). Second, the analysis here focuses on a once-a-decade cycle, when the district boundaries of the past election would almost certainly differ from the five to come. In years when a reapportionment-induced change in constituency has not occurred, we expect that the member's recent electoral margin would provide information that is more reliable and therefore more relevant to the incumbent's perceptions of political vulnerability.

In sum, we find compelling evidence of the disutility that rational incumbents associate with seeking the votes of potentially unsympathetic, perhaps hostile, voters. Under these circumstances, they are less willing to invest in the various costs of a campaign with a relatively low probability of a satisfactory election return. All three variables that we use to tap this factor prove statistically significant, consistently nontrivial, and sometimes dramatic in their effects on retirement probabilities. Seekers of reelection, it seems, do not much like to seek what they are unlikely to find.

CONCLUSION

The single most important source of members' departures in the postwar House has been their voluntary retirements, not their involuntary defeats. Given the several advantages that incumbents (still) enjoy in the electoral arena, such retirements, in turn, not only affect electoral competitiveness district by district; they also affect the potential for change in the collective composition and perhaps (the democratic theorist hopes) in the responsiveness of the chamber to public desires for policy change (e.g., Arnold 1990; Brady 1988; but see Achen 1978). To understand these larger processes better, however, we must comprehend the individual decisions upon which patterns of voluntary turnover depend. To that end, we develop and test a theory of incumbent legislators' decisions to run for or retire from the House, a theory we subsume under the alliterative label "avarice and ambition."

We begin by assuming that a member of Congress, like any individual contemplating retirement options, acts with an eye to his or her financial interests. We do not assume, however, that all financial calculations are of a piece, and we specify a model in which several different types of financial interest operate separately and differently. In deciding to run or retire in a given year, we find, members *do* look ahead to the implications for their postretirement income stream. But we find little evidence that the loss of honoraria or the last-chance opportunities for campaign cash windfalls were significant factors in members' decisions.

These findings are important in several respects. Members may be avaricious, but not in the cynical sense that Mark Twain said and more contemporary critics assume. Undoubtedly there will be scalawags sufficient in number (i.e., n > 0) to justify ongoing media and good-government group scrutiny. But our results regarding members' systematic behavioral tendencies do not sustain the nearly global cynicism apparent in public opinion polls, public interest group critiques, and numerous press commentaries. Moreover, our findings regarding the behavioral importance of unseemly greed contradict those of the recently emerging scholarship on this question, most notably that of Groseclose and Krehbiel (1994; see also Ahuja et al. 1993). Our different results highlight the need to specify a model of career choice that is embedded in a theory of both the long- and short-run calculations of strategic actors. Honoraria and campaign cash may provide spontaneous gratification, financially speaking. But the Constitution does require that House members be 25, old enough (we hope) to know that one must sometimes sublimate infantile tendencies to one's longer-term interests.

In sum, we take the ethical implications of the three hypothesized forms of financial self-interest in Congress to be qualitatively different. By itself, the idea that members of Congress care about their personal finances is not a tendency to be cynically denigrated. To do so would be to establish a standard so strict that it renders all but the most ascetic critic of Congress merely hypocritical. Rather, the source of cynicism should be *how* members go about pursuing financial gain. Using a House Post Office staffer to help one embezzle funds, accepting bribes with "larceny in one's heart," or explicitly trading legislative favors to expand one's campaign warchest—all of these are patently unethical acts. So, too—congressional outsiders and insiders now agree—is the acceptance of a two-thousand-dollar honorarium for little more than a 20-minute speech. And so too—outsiders and insiders agree—is the conversion of campaign contributions to the personal wealth of the retiring member. That the timing of retiring from one's job is affected by the calculation of one's postretirement income, however, is a tendency to which we are all quite legitimately inclined, whether we be part of the federal pension system, contemplating options in a private retirement plan, or deciding whether to retire at age 62 or 65 for the purpose of collecting Social Security benefits.

Moving from ethical standards to behavioral implications, the account of financial incentives embedded in our theory is noteworthy in a second respect. Simply put, it should generalize beyond the decision context of any particular election cycle. Golden parachutes, we would simply point out, are gone with the 1992 wind. Or, to switch to a different movie metaphor, the Wizard's balloon has left for Kansas, never to return to Oz. So too with honoraria income. That witch was made to melt in 1989 as well. History shows, however, that congressional pay raises perennially appear on the legislative agenda, and pay raises immediately generate increases in House members' pension annuities. In the Congress-by-Congress aggregates, in turn, the implication is that we should see nonincremental increases in voluntary retirements in the lagged aftermath of nonincremental pay raises. In the cross-sectional analysis of individual members in a single Congress, in any case, member benefits are highly variable and thus stand to illuminate, in years other than 1992, the choices of individual incumbents to run once again or give up their seats.

The final departure of the approach developed here is our account of intrainstitutional ambition. To switch back to Weber's language, legislators may "live off" politics (at least in part), but do they live "for politics"? We begin by assuming that they do and attempt to imbue the concept of intrainstitutional ambition with behavioral meaning. We embed in our theory of choice a set of calculations through which members attribute value to both the positions they currently hold (and expect to retain) and the positions (with variable probability) they might soon obtain. Again, the lesson is that in making career decisions, they consider both the short- and long-run implications of alternative choices. On this point, too, we find evidence in support of our theory. For those in a position to or poised soon to achieve a position through which they can exercise significant legislative advantages in particular domains—namely, those well situated in one of several majority party queues—their probability of retirement is consistently lower, sometimes dramatically so.

The same cannot be said of either minority party leaders or (ambitious) backbenchers. Our results suggest that the expected utility associated with high standing in a House minority queue is sufficiently low as to exert no predictable difference in the career decisions we have analyzed here. While some within the minority may be disaffected, a significant increase in the probability of minority members' retiring appears only among committee leaders, those for whom the opportunities for upward mobility have reached their limit.

Building on the theoretical account and the evidence that supports it, then, we see several fruitful lines of research that might enhance our understanding of intrainstitutional ambition and its electoral consequences. One is to incorporate a more explicit account of the incentives and opportunity structure governing upward mobility within the majority and minority party organizations. While the empirical literature on legislative party leadership is extensive (e.g., Cox and McCubbins 1993; Rohde 1991), we know of no systematic attempts to model the intraparty career ambitions of legislative partisans. Likewise, the arguments and evidence presented here recommend greater attention to an analogous need to model the strategic choices of progressively ambitious members (differently) situated in multiple (more or less desirable) institutional queues. Research along these lines, we believe, will open up the systematic study of career advancement in Congress in a way that will help us to understand the unequal advantages that nominal equals come to enjoy, legislative game by legislative game.

Our final set of results goes to the importance of member calculations regarding electoral vulnerability in estimating their static ambition (i.e., the value they attach to their current job versus the costs associated with a campaign to keep it). In this part of our study, we reinforce the general findings of several recent papers (Ahuja et al. 1993; Groseclose and Krehbiel 1994; Kiewiet and Zeng 1993) and a longer line of theoretical work on the strategic choices of congressional candidates (e.g., Jacobson and Kernell 1983). We find that reputational damage short of a personal scandal can significantly alter member estimates of the subjective costs associated with a reelection campaign. Other electoral considerations matter as well, with our results providing additional evidence that both an increase in unsympathetic constituents induced by reapportionment and the thinness of past electoral margin contribute to members' fear of facing the voters yet another time.

Thus does the study of retirements meet the large and important literature on candidates for office. As Jacobson and Kernell (1983) have argued, the most important factor in determining electoral competitiveness (and hence involuntary turnover) is the quality of the challenger. And when incumbents are weakened, through any number of mechanisms, potentially strong challengers are more likely to run. This lesson is not lost on sitting members, who continually scan the horizon for the identities and attributes of those who might run against them. The sight of strong opponents nearing the race, in turn, only reinforces the electoral fears identified here, and the "invincible incumbent" miraculously discovers he has a family, to whom he must devote more time and attention.

Members of Congress, in sum, may desire reelection. Such is the irreducible element of political ambition. But their desire is not unbridled; they will not go anywhere or pay any price to seek something they may not find. In their desire for reelection, moreover, members harbor a postelection purpose. They seek policy choices to their respective likings, and to that end they seek positions that might give their preferences preferential treatment. But here, too, their purposes are qualified by expectations of success, and those expectations are governed by the positions they hold or might soon hold in an institutional queue that makes a legislative difference. Finally, the passions of politicians include avarice as well as ambition, the desire for money as well as the desire for power. But whether it be due to the angelic side of their nature or "auxiliary precautions" as Madison put it, we find that instances of overweening greed in Congress appear at most among the few.

APPENDIX

Honoraria Income. The effect of the salary increase enacted in 1989 was diminished by the variable opportunity cost of the honoraria ban. Given that pay raises are constant across members, we simply include as a measure of the ban's opportunity cost the honoraria income that member *i* received in 1989, the last full year before the reform, as reported by *Congressional Quarterly Weekly Report* ("For the Record" 1990).

Convertible Campaign Funds. Contrary to the assumption evident in numerous press reports as well as a recent test of the golden parachute hypothesis (Groseclose and Krehbiel 1994), FECA grandfathers who retired by 1992 were *not* permitted to keep whatever cash they happened to have at the moment of retirement. Rather, the 1989 reform that terminated the grandfather clause stipulated that members could convert to their personal use *no more than* the campaign cash on hand when the reform package was enacted in late 1989. Hence, the maximum value of the parachute's gold was the 1989 imposed cap or the 31 December 1991 cash on hand, *whichever was lower*. For some members, in fact, their campaign assets at the time of retirement were hundreds of thousands of dollars less than the 1989 imposed ceiling, thereby creating some valueless glitter, which was then unwittingly highlighted in most press accounts of the time. Data on the legal level of grandfathered members' maximum convertible cash and their 31 December 1991 cash on hand was collected from FEC records.

Pension Benefits. Unlike the honoraria and campaign cash variables, pension benefits do not accrue at (or near) the moment of retirement but are paid out over some period of years. In order to compare the several variables, then, we must calculate for 1990, 1992, and 1994 the present value of the pension annuity for member *i*. To do that, two factors come into play:

I. The legal formula in place for calculating a particular member's annual pension payment or yearly "cash flow" (C), specified below.
II. Constraints on the annual pension benefit.
    1. It is capped at 80% of a member's highest yearly salary.
    2. The number of years before a member is eligible to receive benefits (y) is governed by federal pension regulations regarding age and years of service (s):
        i. Members are ineligible for pension benefits if $s < 5$.
        ii. Members born before 1948 for whom $5 < s < 10$ are eligible to receive benefits at age 62.
        iii. Members born before 1948 for whom $10 < s < 20$ are eligible at age 55.
        iv. Members born after 1948 for whom $10 < s < 20$ are eligible at age 56.
        v. Members born after 1948 for whom $s \geq 20$ are eligible at age 50.
        vi. Members for whom $s \geq 25$ are eligible at any age.

To produce the appropriate present value estimate, two additional factors must also enter the calculation. The first is the (computationally convenient) fact that congressional pension benefits, unlike congressional salaries, receive an annual cost-of-living adjustment. The second is the life expectancy of the member in year *j*, given the member's age, gender, and race. Life expectancy estimates are taken from the U.S. Bureau of the Census (1992, 77).

To estimate the pension annuity present value for member *i* retiring in year *j*, then, we use the standard present value formula (see, e.g., Brealey and Myers 1985), subject to the constraints on C and y already discussed:

$$PPV_{ij} = \frac{C}{r}\left(1 - \frac{1}{(1+r)^t}\right)\left(\frac{1}{(1+r)^y}\right)$$

where PPV_{ij} is the pension annuity present value for member *i* retiring in year *j*; C is the annual cash payment to eligible member *i* in year *j*, which by law is equal to the product of (1) the average of member *i*'s three highest yearly salaries, (2) the number of member *i*'s years of service, and (3) the constant .025 (this product adjusted annually for inflation); r is the expected rate of return, which we here assume to be 2%, given that C is automatically adjusted for inflation; t is the life expectancy of member *i* in year *j*; y is the number of years before member *i* becomes eligible to receive benefits.

To capture the changing financial incentive to retire one cycle rather than the next, we then calculate the *increase* in the pension present value for 1992 versus 1990 and 1994 versus 1992 pension benefits; that is, $PV_{i92} - PV_{i90}$ and $PV_{i94} - PV_{i92}$.

Assuming diminishing marginal utility as the value of a particular benefit goes up, finally, we take the natural log of each of the several financial variables.

Notes

We would like to thank Steve Ansolabehere, Paul Courant, Tim Groseclose, John Huber, John Jackson, John Kingdon, Keith Krehbiel, Gary McKissick, Barry Weingast, Tim Wells, and Dave Wright for comments and assistance on earlier drafts.

1. That the bargain was explicit and simultaneous is captured in the provision that the ban on honoraria income for House members would become automatically void if the 25% pay raise in 1991 was subsequently repealed (*Congressional Quarterly Almanac* 1989, 52).

2. Members considering delaying their retirement from 1990 to 1992, however, could count on a substantial pension increase with greater certainty, in that the largest salary increase (25%) was slated for 1991. More importantly, protections against a post-1990 pay raise scaleback were built into a noteworthy proviso included in the original compromise package itself, which stipulated that if the 1991 pay increase was subsequently repealed, the ban on honoraria income would become automatically void (*Congressional Quarterly Almanac* 1989, 52).

3. Shepsle develops an analogous logic in his model of nonfreshman transfer requests (1978, chap. 5). Members' incentives to transfer from a current to a more preferred committee are diminished by the expected loss in proximity to the top. To use Shepsle's language, the "wealth" of a seat on some other committee must be discounted by the opportunity costs of queue switching, costs that grow rapidly as the member ascends over time toward a leadership position.

4. Kiewiet and Zeng (1993) model the choices House members might simultaneously make among all three options: to retire, to run for reelection, or run for higher office. In contrast, our focus here is on the pairwise choice of

running for reelection or retiring from office (see also Groseclose and Krehbiel 1994). This for two reasons. First, we disagree with the Kiewiet and Zeng premise that "most of the time congressmen actually face all three choices—run for reelection, retire, or run for higher office—simultaneously" (1993, 928). In any given election year, only a small proportion of House members have a remotely viable shot at higher office, such that choice in principle is no choice in practice. Second, to run again or retire from office is a qualitatively different type of choice than the decision to run for a currently held versus a higher office. A departure from the House in the first case more likely implies a diminution of political ambition and a return to private life; in the second, it implies an expansion of political ambition and a search for even greater public distinction. Thus there is little basis for assuming that the factors appropriately specified to affect the former should be the same as those specified to affect the latter.

5. In determining whether a member is second in line, we rely on a qualified seniority assumption. If the member is officially listed as second in committee (within-party) seniority, he or she is counted as second in line except if (1) that member has already been deposed or otherwise repudiated in a bid for the relevant committee leadership position or (2) that member has had the option of leading the committee but has chosen not to take it, in favor of a leadership position elsewhere. Where either of those conditions hold, we count as second in line the next member in the seniority queue who meets neither. When a member was second in line for more than one committee, we used the lowest proximity score divided by two so as roughly to reflect the fact that such a member had two shots at a leadership position, whereas the typical second-in-line member had only one. Finally, we excluded select, special, and joint committees, as well as those that rotated membership and leadership positions, such as Budget and Standards of Official Conduct.

6. A plausible alternative is to create a vector of variables, including age and electoral vulnerability factors, that might reflect the information that aspirants process regarding the first-in-command's likelihood of departure relative to their own. Such a measure would imply a considerably more information-rich and sophisticated calculation by the progressively ambitious member, however. Still, alternative measurement strategies warrant greater attention in future research.

7. The weak coefficient on subcommittee chair may reflect the fact that being a subcommittee chair, *any* subcommittee chair, quickly reaches a point of diminishing marginal returns in terms of ambition realized. What matters more is the variable probability of moving to a subjectively better subcommittee chairmanship, something we could not capture here. (In this respect, we violate our own expected-value argument.) There is, for instance, considerable turnover in subcommittee chair holdings, which should evoke calculations of progressive ambition short of moving up to full committee chair. In addition, there is reason to worry that the negligible effect of this variable (and the substantively modest effects of the other two) may also reflect the crude way in which we measure the position's value. Not all subcommittees are alike in terms of their scope and jurisdiction (even within a single committee), just as all committees are not alike in terms of theirs (Fenno 1973; King 1994). Better measurement is needed; but progress on this score is inherently difficult, given the subjective—and hence variable—nature of individuals' estimates of any particular position's value (Shepsle 1978).

8. The principal categories are adapted from or contained in the FEC reports, though the FEC summaries for each candidate rely on candidate classifications of their own itemized disbursements. We scrutinized each disbursement for all 32 members and corrected for what turned out to be a small number of obvious misclassifications.

9. Most of the remainder went to "operating expenses," which includes some disbursements that appear to provide indirect subsidies to former members. However, even were one to assume (1) that *all* of the questionable disbursements in fact supplemented former members' disposable incomes indirectly (i.e., substituted for what otherwise would have been out-of-pocket expenses for a typical private citizen) and (2) that some portion of the charitable contributions may reflect substitution effects (i.e., that members who would otherwise make charitable contributions from their own personal income contributed from their campaign funds instead), there still exists no reasonable accounting such that members derived personal financial benefits close to a third as large as their convertible cash on hand. For instance, retired House members exhibit rates of giving leftover campaign funds to charities and nonprofits at least a dozen times the rate of private citizens with incomes of $100,000 or more (U.S. Bureau of the Census 1992, 375).

10. Note that this result obtains, controlling for member age—a variable that proved correct in sign and statistically and substantively significant. Treating retirement probability as a quadratic function of age, likewise, does not affect the results that we report.

References

Achen, Christopher. 1978. "Measuring Representation." *American Journal of Political Science* 21:805–15.

Adams, John. 1954. *The Political Writings of John Adams.* Indianapolis: Bobbs-Merrill.

Ahuja, Sunil, and Staci Beavers, Cynthia Berreau, Anthony Dodson, Patrick Hourigan, Steven Showalter, Jeff Walz, and John R. Hibbing. 1993. "Modern Congressional Election Theory Meets the 1992 House Elections." University of Nebraska, Lincoln. Typescript.

Arnold, R. Douglas. 1990. *The Logic of Congressional Action.* New Haven: Yale Press.

Brady, David W. 1988. *Critical Elections and Congressional Policy Making.* Stanford: Stanford University Press.

Brealey, Richard, and Stewart Myers. 1985. *Principles of Corporate Finance.* New York: McGraw-Hill.

Chaffee, Kevin. 1991. *Saving for a Rainy Day: How Congress Turns Leftover Campaign Cash into "Golden Parachutes."* Washington: Center for Public Integrity.

Cheney, Richard. 1989. "An Unruly House: A Republican View." *Public Opinion,* Jan.–Feb, pp. 41–44.

"Congress Hikes Pay, Revises Ethics Law." *Congressional Quarterly Almanac* 1989: 51–60.

Cooper, Joseph, and William West. 1981. "Voluntary Retirement, Incumbency, and the Modern House." *Political Science Quarterly* 96:279–300.

Cox, Gary W., and Matthew D. McCubbins. 1993. *Legislative Leviathan.* Berkelye: University of California Press.

Fenno, Richard F., Jr. 1973. *Congressmen in Committees.* Boston: Little, Brown.

Foerstel, Karen. 1992. "In Surprise, Rep. Coughlin (R-PA) To Retire." *Roll Call,* 24 February, p. 7.

"For the Record." 1990. *Congressional Quarterly Weekly Report,* June, pp. 1750–53.

Groseclose, Timothy, and Keith Krehbiel. 1994. "Golden Parachutes, Rubber Checks, and Strategic Retirements from the 102nd House." *American Journal of Political Science* 38:75–99.

Hibbing, John R. 1982. "Voluntary Retirement from the U.S. House of Representatives: Who Quits?" *American Journal of Political Science* 30:651–65.

Jacobson, Gary, and Samuel Kernell. 1983. *Strategy and Choice in Congressional Elections.* New Haven: Yale Press.

Kiewiet, D. Roderick, and Langche Zeng. 1993. "An Analysis of Congressional Career Decisions, 1947–1986." *American Political Science Review* 87:928–41.

King, David C. 1994. "The Nature of Congressional Committee Jurisdictions." *American Political Science Review* 88:48–62.

Kingdon, John W. 1968. *Candidates for Office.* New York: Random House.

Loomis, Burdett. 1988. *The New American Politician.* New York: Basic Books.

Mann, Thomas, 1978. *Unsafe at Any Margin.* Washington: AEI Press.

Ornstein, Norman. 1989. "You Get What You Pay For" *Newsweek,* January 16, 10.

Polsby, Nelson. 1968. "The Institutionalization of the House of Representatives." *American Political Science Review* 62:144–68.

Renfro, William. 1992. "Green Pastures." *Atlantic Monthly,* February, pp. 24–25.

Rohde, David W. 1991. *Parties and Leaders in the Postreform House.* Chicago: University of Chicago Press.

Schlesinger, Joseph. 1966. *Ambition in Politics: Political Careers in the United States.* Chicago: Rand McNally.

Shepsle, Kenneth A. 1978. *The Giant Jigsaw Puzzle.* Chicago: University of Chicago Press.

Smith, Adam. 1801. *The Theory of Moral Sentiments.* 9th ed. London:

Twain, Mark. 1873. Letter to the Editor. *New York Daily Tribune,* 10 March, p. 5.

U.S. Bureau of the Census. 1992. *Statistical Abstract of the United States 1992.* Washington: GPO.

Weber, Max. 1965. *Politics as a Vocation.* Philadelphia: Fortress.

Richard L. Hall is Associate Professor of Political Science and Public Policy, University of Michigan, Ann Arbor, MI 48109-1045.

Robert P. Van Houweling is a Legislative Assistant in the Office of Senator Thomas A. Daschle, United States Senate, Washington, DC 20510.

American Political Science Review Vol. 89, No. 1 March 1995

CONDORCET WINNERS AND THE PARADOX OF VOTING: PROBABILITY CALCULATIONS FOR WEAK PREFERENCE ORDERS

BRADFORD JONES *University of Arizona*
BENJAMIN RADCLIFF *University of Notre Dame*
CHARLES TABER and RICHARD TIMPONE
State University of New York, Stony Brook

That individual preferences may be aggregated into a meaningful collective decision using the Condorcet criterion of majority choice is one of the central tenets of democracy. But that individual preferences may not yield majority winners is one of the classic findings of the social choice literature. Given this problem, social choice theorists have attempted to estimate the probability of Condorcet winners, given certain empirical or theoretical conditions. We shall estimate the probabilities of Condorcet winners and intransitive aggregate orders for various numbers of individuals with strong or weak preference orders across various numbers of alternatives. We find, using computer simulation, a stark contrast between these estimates assuming strong individual preferences and the estimates allowing for individuals' indifference between pairs of alternatives. In contrast to earlier work, which depends on the strong-preference assumption, we suggest that the problem is most acute for small committee decision making and least acute for mass elections with few alternatives.

As conventionally understood, democracy implies a process of reaching social decisions via the principle of majority rule. Political decisions are putatively made in accordance with the preferences of citizens as a collective entity. Collective preferences, in turn, are determined by an aggregation of individual preferences, wherein the collective choice is that option which is majority-preferred to all other options in pairwise comparisons; such a majority choice is called a Condorcet winner. More than anything else, it is this emphasis on majority rule that underlies both the liberal (e.g. Locke) and radical (e.g. Rousseau) interpretations of democracy.

Unfortunately, this simple vision of majority rule is plagued by deep and seemingly ineradicable theoretical difficulties. As the social choice literature documents, Condorcet winners need not exist (Arrow 1963).[1] Simply put, there need be no alternative that is majority-preferred. If so, then voting games would appear capable of producing results that are not consistent with the majoritarian principle. This phenomenon has been known for centuries. In fact, awareness of the problem has affected practical politics, from Pliny the Younger's manipulation of the sentencing agenda in the case of the murdered consul Afranius Dexter (Farquharson 1969) to tantalizing speculations that Thomas Jefferson and James Madison may have been influenced by Condorcet's work on social choice in their views of governmental structure (McLean and Urken 1992; Miller and Hammond 1989).[2]

Of course, the various impossibility theorems about group choice suggest not that Condorcet winners never exist, but only that they are not guaranteed. In other words, majority-preferred alternatives are not assured, though they may sometimes exist. Given that fact, a great deal of attention has been paid to the question of the frequency of Condorcet winners.[3] One approach has been to search for empirical examples of Condorcet winners in real-world situations, such as presidential elections (Niemi and Wright 1987; Radcliff 1994), legislatures (Riker 1958), university elections (Niemi 1970), and professional associations (Chamberlain, Cohen, and Coombs 1984). Another has been to examine the a priori probability of majority winners (devoid of any particular institutional or empirical setting) by making certain simplifying assumptions about the preferences of individuals (DeMeyer and Plott 1970; Garman and Kamien 1968; Klahr 1966; Niemi and Weisberg 1968).

While the latter studies deservedly have become classics within the public choice literature, each suffers from the fact that they consider only "strong" preferences. In other words, they begin with the assumption that individuals have strict preferences over each pair of alternatives, such that indifference relations are not admissible. The difficulty is that this assumption is purely analytic: it is not driven by any empirical evidence on the structure of preferences but rather by the daunting complexity of the problem. Since that complexity increases dramatically as the number of possible (transitive) preference orders increases, the appeal of strong orders as a simplifying assumption is obvious. The need to reduce complexity was particularly compelling, given the technological limitations of the period in which much of this work was completed (e.g., DeMeyer and Plott 1970; Klahr 1966).

TABLE 1

Strong Preferences, Probability of No Majority Winner ($1 - Q(m, n)$)

VOTERS (n)	NUMBER OF ALTERNATIVES (m)												
	3	4	5	6	7	8	9	10	11	12	13	14	15
3	.056*	.111*	.160*	.204*	.239*	.271*	.299*	.324*	.346*	.366*	.386*	.402*	.417
5	.070*	.139*	.200*	.252*	.296*	.333*	.367	.397	.423	.445	.467	.487	.504
7	.075*	.150*	.216*	.270*	.318*	.358*	.395	.426	.452	.478	.500	.519	.536
9	.078*	.156*	.224*	.282*	.330	.373	.409	.440	.468	.495	.516	.535	.554
11	.079*	.160*	.228*	.288*	.338	.380	.418	.448	.478	.504	.526	.547	.565
13	.081*	.162	.232	.292	.343	.386	.424	.455	.484	.510	.533	.554	.572
15	.082*	.164	.235	.295	.346	.390	.428	.460	.489	.515	.539	.559	.577
17	.083*	.165	.237	.298	.349	.393	.430	.464	.493	.519	.541	.561	.581
19	.083*	.167	.239	.300	.351	.395	.432	.466	.496	.522	.544	.565	.584
25	.084*	.169*	.242*	.303*	—	—	—	—	—	—	—	—	.587
31	.085*	.171	.243	.306	.359	.403	.442	.476	.505	.532	.555	.574	.594
49	.086*	.172*	.245*	.309*	—	—	—	—	—	—	—	—	—
101	.087	.174	.248	.314	—	—	—	.484	—	—	—	—	.604
501	.088	.174	.251	.316	—	—	—	—	—	—	—	—	—
1,001	.088	.176	.251	.316	.368	.413	—	—	—	—	—	—	—
10,001	.088	.176	.252	—	—	—	—	—	—	—	—	—	—

Note: Rows represent the number of voters selecting, while the columns represent the number of options being selected. $1 - Q(m, n)$ is estimated as the proportion of the one million simulated elections that failed to produce a Condorcet winner.
* indicates that our estimate replicates a prior result reported by DeMeyer and Plott (1970) or Ordeshook (1986).

In the last decade, there has been enormous progress in both computer hardware and the conceptual tools of computational modelling (Taber and Timpone 1994). In short, it is now feasible to relax the assumption of strong preferences.[4] Equally important, recent empirical evidence suggests that indifference relations are quite common, at least in the context of some mass voting games (Brady and Ansolabehere 1989; Radcliff 1993). Given that we thus have tools that render the strong-order assumption unnecessary, as well as evidence that it is empirically unrealistic, we provide results of computer simulations of the structure of collective preferences that do not depend upon strong individual preferences.

PRIOR RESEARCH

Given n voters ($n \geq 3$) and m alternatives ($m \geq 3$), there are two quantities of interest: (1) the probability of a Condorcet winner, $Q(m, n)$, and (2) the probability of a transitive, strongly ordered social preference structure, $P(m, n)$.[5] Two research strategies have been used to estimate these probabilities. The first and most obvious is to derive analytic expressions and compute the resulting values for a given m and n (e.g., DeMeyer and Plott 1970). While the advantage of the analytic solution is obvious, it quickly becomes intractable for large values of m or n. This problem is doubly pronounced when abandoning the assumption of strong preferences, in that the number of possible preference profiles becomes extremely large as m increases.

Accordingly, we have turned to the second approach: computer simulation (e.g., Klahr 1966). With this method, one generates a preference profile for each of the n individuals (selected according to a uniform probability distribution from the set of all possible transitive preference profiles for the m alternatives), stores each individual's preferences for all pairwise comparisons over the given alternatives, and computes the resulting social order. Given x iterations of this procedure, one may estimate $Q(m, n)$ to be the proportion of the x simulated elections that yield a Condorcet winner, and $P(m, n)$ to be the proportion that produce a fully transitive strong social order. Given the emphasis on the absence of these outcomes, the values ($1 - Q(m, n)$) and ($1 - P(m, n)$) are typically reported.

Table 1, which replicates and extends prior findings, reports the probability of no Condorcet winner ($1 - Q(m, n)$) for strong orders (for comparison to prior work, see Ordeshook 1986, 58; Riker 1982, 122). As is apparent, Condorcet winners become less likely as (1) the number of alternatives increases and (2) the number of individuals increases. As one reads either down each column (meaning more voters for a given number of alternatives) or across each row (meaning more alternatives for a set number of voters), probabilities uniformly increase. Observing that "there is an apparent limit of one as n and m approach infinity," Ordeshook reasonably concludes that "in large societies confronted with numerous alternatives, the likelihood that [Condorcet winners do not occur] is great" (1986, 58).

Estimates of ($1 - P(m, n)$), or the probability of an intransitive or weak social order, are provided in Table 2 (again, limiting our analysis to strong individual orders). Qualitatively, these results repeat those just described. But, as one would expect, the probability of an intransitive social ordering is always greater than or equal to the probability of no Con-

TABLE 2

Strong Preferences, Probability of Intransitive or Weak Social Order ($1 - P(m, n)$)

VOTERS (n)	NUMBER OF ALTERNATIVES (m)								
	3	4	5	6	7	8	9	10	15
3	.056*	.171*	.324	.491	.641	.767	.858	.918	.998
5	.070*	.211*	.393	.580	.736	.849	.922	.963	.999
7	.075*	.225	.418	.608	.764	.874	.939	.974	1.00
9	.078*	.234	.431	.626	.780	.885	.946	.977	1.00
11	.079*	.240	.439	.634	.788	.892	.951	.980	—
13	.081*	.242	.445	.640	.794	.899	.953	.981	—
15	.082*	.245	.449	.646	.797	.899	.955	.983	—
17	.083*	.247	.453	.648	.801	.901	.957	.983	—
19	.083*	.249	.454	.651	.801	.902	.957	.984	—
31	.085*	.253	.463	.660	—	—	—	—	—
101	.087	.253	.470	.667	—	—	—	—	—
501	.088	.259	.478	.670	—	—	—	—	—
1,001	.088	.260	.477	.673	—	—	—	—	—
10,001	.088	.258	.474	—	—	—	—	—	—

Note: Rows represent the number of voters selecting, while the columns represent the number of options being selected. $1 - P(m, n)$ is estimated as the proportion of the one million simulated elections that failed to produce a strong and fully transitive social ordering.
* indicates the value confirms that reported in DeMeyer and Plott (1970).

dorcet winner, in that we now consider all intransitivities rather than merely top cycles. The former quantity is also more sensitive to increases in m and n, such that the probability of a strong transitive aggregation of individual preferences rapidly approaches zero as the size of the electorate and the number of options increase.

NEW ESTIMATES FOR WEAK ORDERS

Our new simulation follows this same logic but assigns to voters a preference order chosen equiprobably from all transitive profiles, including those containing indifference relations.[6] For example, with three alternatives there are 13 possible orders rather than the six when considering only strong orders. The simulation program is described in an Appendix.[7]

Table 3 reports the new estimates for the probability of no Condorcet winner, denoted ($1 - Q^*(m, n)$). Immediately we can see that one of the classic findings remains unchanged: Condorcet winners still become less likely as the number of alternatives increases. But the second classic result—that Condorcet winners uniformly become less likely as the number of voters increases—does not remain when we allow individual indifference. Indeed, there are several important differences between the estimates in Tables 1 and 3. First, as might be expected, the probability of no majority winner is often greater when we allow indifference, but only for small n and m. For three voters and three alternatives, the probability increases by a factor of six (from .056 to .336); for seven voters and five alternatives, a Condorcet winner is almost twice as unlikely when indifference is involved (.418 vs. .216).

Significantly, the difference decreases as the number of individuals increases, suggesting a second and more interesting finding. In Table 1, which assumes strong preferences, the probability of no Condorcet winner increases monotonically with the number of individuals. Table 3 shows, however, that when we allow weak preferences, the probability is at a maximum when the number of alternatives equals the number of individuals.[8] For any number of options, the probability increases as the number of voters increases, but only until n equals m. It then *falls monotonically*.

It thus appears that the probability of no majority winner is quite large for a small number of voters (especially with many alternatives) but decreases considerably as the number of voters increases relative to the number of options. Thus, Condorcet winners are more likely when the ratio of individuals to alternatives is great (as in mass elections). However, in smaller groups (such as committees), there is much less chance that a majority winner will be attained.

In sum, relaxing the assumption of strong preferences radically alters our deductions about the general probability of Condorcet winners, at least in regard to the size of the electorate. With the assumption of strong orders, the probability of no majority winner increases with the number of individuals or alternatives, though it remains fairly small for few alternatives. Following these findings for strongly ordered individuals, one might conclude that the issue is most pronounced in "large societies confronted with numerous alternatives" (Ordeshook 1986, 58). By contrast, allowing individual indifference, the probability of no majority winner is largest for committees or small electorates but decreases substantially with larger electorates, eventually becoming smaller than when we assume strong preferences. Figure 1, which directly compares the trends

TABLE 3

Weak Preferences, Probability of No Majority Winner ($1 - Q^*(m, n)$)

VOTERS (n)	NUMBER OF ALTERNATIVES (m) 3	4	5	6	7	8	9	10	15
3	.336	.384	.420	.448	.473	.492	.508	.524	.580
5	.309	.377	.428	.467	.500	.528	.549	.568	—
7	.280	.358	.418	.466	.504	.535	.562	.582	—
9	.259	.340	.405	.458	.500	.535	.563	.587	—
11	.242	.324	.393	.448	.493	.531	.562	.588	—
13	.227	.311	.382	.438	.486	.526	.559	.586	—
15	.217	.300	.370	.430	.478	.520	.555	.583	—
17	.207	.290	.361	.421	.472	.514	.551	.579	—
19	.200	.282	.354	.414	.465	.509	.546	.576	—
31	.171	.250	.323	.382	—	—	—	.555	—
101	.120	.195	.265	.327	—	—	—	—	—
501	.082	.155	.222	.286	—	—	—	—	—
1,001	.074	.145	.212	.275	—	—	—	—	—
10,001	.060	.127	—	—	—	—	—	—	—
100,001	.056	.123	—	—	—	—	—	—	—
1,000,001	.056	—	—	—	—	—	—	—	—

Note: Rows represent the number of voters selecting, while the columns represent the number of options being selected. $1 - Q^*(m, n)$ is estimated as the proportion of the one million simulated elections that failed to produce a Condorcet winner.

in Tables 1 and 3 for $m = 3$, illustrates this point. Based on these trends, we might suggest that the problem is actually more relevant to small groups than large societies.[9]

Thus, when n is relatively small (less than 501 for $m < 7$),

$$(1 - Q^*(m, n)) > (1 - Q(m, n)),$$

but when n is relatively large (greater than 501 for $m < 7$),

$$(1 - Q^*(m, n)) < (1 - Q(m, n)).$$

For fewer than seven alternatives, these trend lines cross before we reach 501 voters. Moreover, this result probably holds for larger numbers of alternatives, but computational constraints force us to extrapolate from the data for this conclusion.[10] Increasing the number of alternatives increases both sets of estimates, though those based on strong orders appear to be more sensitive to changing m than those based on weak preferences.[11]

Comparing Tables 2 and 4 suggests a similar pattern for $(1 - P^*(m, n))$ and $(1 - P(m, n))$. The probability of weak or intransitive social orders based on strong individual preferences is smaller than those allowing individual indifference for small n, but the former increases with larger n while the latter decreases.[12]

To this point, we have treated all preference profiles as equiprobable in both sets of simulations. But since there are more possible weak than strong preference orders (e.g., 7/13 orders are weak for $m = 3$, and the discrepancy grows with m), this assumption may actually overweight the probability of indifference. At a minimum, we must check to see how sensitive our basic findings are to this problem. More generally, it might be substantively interesting to explore the impact of varying the amount of indifference in the electorate.

We conducted a third simulation analysis in which we manipulated the probability of selecting a strong or weak profile, denoted W. The set of possible preference orders for a given m was divided into a strong subset and a weak subset. Each individual was then assigned to either a "strong" or "weak" condition, according to a probability W. Those in the strong category (i.e., they have preferences across all pairs of alternatives) were then assigned a preference order from the strong subset according to the uniform probability distribution. Those in the weak category were assigned preferences from the set of possible weak orderings, also according to the uniform probability distribution.[13] Note that this procedure allows us to control the amount of indifference present in the

FIGURE 1

Contrasting Trends for Strong Versus Weak Preference Orders for Three Alternatives

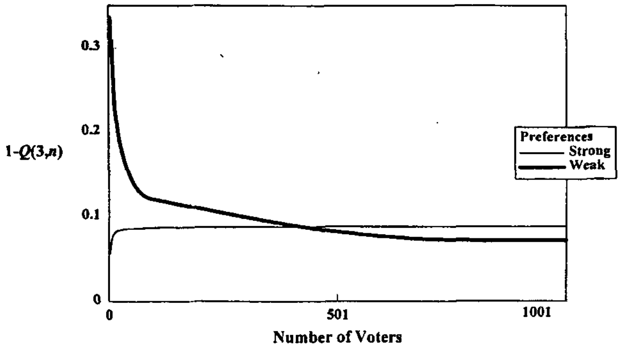


TABLE 4

Weak Preferences, Probability of Intransitive or Weak Social Order ($1 - P^*(m, n)$)

VOTERS	NUMBER OF ALTERNATIVES (m)								
(n)	3	4	5	6	7	8	9	10	15
3	.514	.703	.832	.910	.956	.980	.991	.996	1.00
5	.465	.681	.830	.918	.964	.986	.995	.998	—
7	.421	.649	.816	.915	.965	.987	.996	.999	—
9	.388	.621	.799	.907	.963	.987	.996	.999	—
11	.362	.595	.782	.899	.960	.986	.996	.999	—
13	.341	.572	.766	.889	.956	.985	.995	.999	—
15	.323	.553	.751	.881	.952	.983	.995	.999	—
17	.308	.537	.736	.873	.948	.982	.995	.999	—
19	.297	.522	.725	.866	.944	.981	.994	.998	—
31	.250	.463	.674	.830	—	—	—	.998	—
101	.166	.354	.564	.747	—	—	—	—	—
501	.104	.265	.473	.668	—	—	—	—	—
1,001	.090	.244	.446	.647	—	—	—	—	—

Note: Rows represent the number of voters selecting, while the columns represent the number of options being selected. $1 - P^*(m, n)$ is estimated as the proportion of the one million simulated elections that failed to produce a strong and fully transitive social ordering.

electorate because for n sufficiently large, the individual probability W translates into a proportion of voters w who have weakly ordered preferences. We do not control how much indifference a voter in the weak condition has, however, allowing this to depend on random assignment. Completely indifferent voters are least likely, because there are more possible combinations with fewer indifference relations. Of the seven weakly ordered preference patterns when $m = 3$, for example, only one is totally indifferent ($x = y = z$), while six have one indifference relation (e.g., $x = y > z$). Table 5 gives the results from this analysis for three, four, and five alternatives, with estimates based on one hundred thousand iterations. Note that column 1, where $W = 0$, reports (essentially) the same results as the strong preference estimates in Table 1 and that $W = .54$ generally recreates our weak results from Table 3 for $m = 3$ (7 of 13 alternatives are weak, which translates to $W = .54$). Finally, as described earlier, though 100% of the voters who contribute to the last columns of Table 5 are weakly ordered, only a minority of them have no preference.

For three voters, Condorcet winners become increasingly unlikely as W increases, reaching a maximum probability of .389 when all voters have weak orders. More interesting, however, is the finding that these trends are nonmonotonic for $n > 3$, reaching some maximum before $W = 1.00$ and then declining. Moreover, this maximum estimate for $1 - Q^*(m, n)$ is reached for an ever smaller value of W as n grows larger. With three alternatives and an electorate of 31, for example, Condorcet winners are least likely when each voter has between .10 and .20 chance of being indifferent between at least one pair of alternatives. But 101 voters choosing across three alternatives are least likely to get a Condorcet winner when fewer than 5% of voters are weakly ordered (when $W = .05$, $1 - Q^*(3, 101) = .143$). The chance of getting a majority winner increases steadily from this point as more voters' preference orders contain indifference. And we also find this basic pattern for $m = 4$ or $m = 5$.

Mass electorates ($n \geq 501$) do not have to be unreasonably indifferent for our basic conclusion to hold. When 40% or more of an electorate of 1,001 have weakly ordered preferences, they are *more likely* to get a Condorcet winner than if all have strong preferences. In general, for large n, highly indifferent electorates are most likely to find Condorcet winners,[14] but a lot of indifference is not necessary for majority winners to be more probable than for electorates with strongly ordered voters. This is true for three, four, or five alternatives. On the other hand, committees (n relatively small) with indifferent members are less likely to find Condorcet winners than those with strongly ordered members.

DISCUSSION

In terms of committees (where the number of voters tends to be relatively small), Condorcet winners are much less likely when abandoning the assumption of strong preferences. If committee members tend to be indifferent among some of the alternatives, the probability of achieving a nonarbitrary outcome becomes relatively small. When many alternatives are available, committees will have an even more difficult time finding Condorcet winners. This result suggests that institutions that rely heavily on committee decision making may require special mechanisms designed to create stable and fair decision outcomes (Miller and Hammond 1989; Shepsle 1979).

In terms of mass elections (where the number of voters is by definition extremely large), our results show that Condorcet winners are more likely than when using the strong-order assumption. Grofman (1993), interestingly enough, comes to a similar con-

TABLE 5

Impact on (1 − $Q^*(m, n)$) of Varying the Level of Indifference

	PROBABILITY OF A WEAK PROFILE (W)											
m AND n	.00	.10	.20	.30	.40	.50	.54	.60	.70	.80	.90	1.00
3												
3	.056	.144	.209	.260	.298	.329	.339	.349	.364	.376	.380	.389
5	.070	.173	.229	.269	.294	.309	.307	.311	.314	.315	.314	.314
7	.075	.184	.238	.271	.278	.280	.282	.278	.280	.279	.272	.274
9	.078	.191	.237	.254	.259	.258	.259	.258	.256	.250	.248	.241
11	.080	.194	.231	.241	.244	.242	.243	.241	.235	.231	.227	.222
13	.081	.196	.227	.233	.233	.226	.229	.226	.225	.217	.211	.205
31	.085	.182	.182	.180	.178	.171	.171	.167	.162	.155	.146	.136
101	.087	.141	.137	.131	.129	.122	.120	.116	.108	.098	.089	.077
501	.088	.111	.103	.100	.093	.085	.082	.079	.071	.060	.048	.035
1,001	.088	.102	.096	.090	.085	.078	.074	.070	.062	.050	.039	.025
10,001	.088	.088	.083	—	—	—	.060	—	—	—	—	—
4												
3	.111	—	.229	—	.310	—	—	.368	—	.405	—	.424
9	.156	—	.293	—	.331	—	—	.342	—	.335	—	.323
31	.171	—	.277	—	.266	—	—	.257	—	.241	—	.222
501	.174	—	.193	—	.178	—	—	.163	—	.144	—	.121
1,001	.176	—	.182	—	.169	—	—	.152	—	.133	—	.111
10,001	.176	—	.167	—	—	—	—	—	—	—	—	—
5												
3	.160	—	.256	—	.325	—	—	.384	—	.424	—	.450
9	.224	—	.339	—	.388	—	—	.406	—	.405	—	.398
31	.243	—	.350	—	.347	—	—	.333	—	.320	—	.306
501	.251	—	.268	—	.255	—	—	.239	—	.221	—	.202
1,001	.251	—	.260	—	.242	—	—	.226	—	.211	—	.192

Note: Rows represent the number of alternatives (m) and the number of voters (n), while the columns represent the probability of individuals being assigned a weak preference order. $1 - Q^*(m, n)$ is estimated as the proportion of the one hundred thousand simulated elections that failed to produce a Condorcet winner. Values in the column for .00 replicate the fully strong estimates of Table 1, and values in the column for .54 replicate the weak estimates for m = 3 of Table 3.

clusion, though following a different argument. Observing that empirical examples of majority cycles in mass electorates are nearly as rare as sightings of the Loch Ness monster, he suggests that social choice theorists greatly underestimate the probability of majority winners because they assume that preferences are equally likely, when in fact the distribution of preferences is skewed by political culture and the two-party system.[15] Though we agree that the uniformity assumption is highly suspect, we find that relaxing a different assumption—that of strong preference orders—produces theoretical trends in keeping with his empirical observation. To paraphrase Grofman, when we shot a million Arrow's theorems into the body politic, very few hit anything (1993, 1547)! In other words, fewer simulation runs over mass electorates produced cycles when we allowed for individual indifference.[16] These findings clearly suggest that the democratic method is more likely to function as predicted in majoritarian theory when large numbers participate.

Curiously, that conclusion is quite close to the traditional argument that large numbers of citizens are more likely to make "good" decisions than smaller numbers. Aristotle makes the classical case for this contention in the *Politics*, arguing that the summing of mass opinion may be superior to that of a more select group, even if the elite is, person for person, more knowledgeable or sophisticated than the mass. Similar arguments can also be found in Rousseau's *Social Contract* and Condorcet's "jury theorems" (for discussions in social choice terms, see Grofman and Feld 1988; Young 1988). While our results say nothing about the quality of decisions that the analogy of the jury implies, they do suggest that larger groups are more likely to produce Condorcet winners than smaller ones. Thus the *structure* of social preferences, at least, is more likely to conform to theoretical expectations when more rather than less people take part in voting.

We close with two caveats. First, and most obviously, we do not pretend that the results from this simulation are empirically "realistic" (for a discussion of these limitations in computational modelling, see Taber and Timpone 1994). Like prior scholars of the subject, we recognize that preference profiles are not really uniformly distributed in the electorate, though we still make that simplifying assumption (only partly relaxed when we manipulate the probability of indifference). While this assumption limits the applicability of the findings to real-world situations, it does allow us to determine the a priori propensity for cycling and, more importantly, to better understand how that propensity varies with the number of people choosing and the range of options available to them.

Second, while our work paints quite another picture than did prior studies that only considered strong preferences, we do not mean to imply that ours is categorically superior. The opposing results simply reflect the difference in the enabling assumption regarding the admissibility of indifference relations. The relative utility of the results reported here versus prior findings thus depends entirely upon the empirical question, peculiar to any particular institutional setting, of whether most individuals have strict preferences over each pair of alternatives. Still, though the assumption that indifference relations do not occur may not be genuinely "heroic" in the worst sense of that term, it nonetheless remains empirically suspect and theoretically unnecessary. We thus suggest, in the absence of any empirical reason to believe otherwise, that our estimates are likely to prove more useful.

APPENDIX

Social Choice 4.0 (SC4) and Weak Social Choice 5.0 (WK5) are computer programs (written in C and compiled for an IBM-compatible personal computer) that estimate the probabilities of getting a majority winner ($Q(m, n)$) and of getting a transitive social ordering ($P(m, n)$) for different electorate sizes (n) and numbers of vote alternatives (m). SC4 assumes that all voters have strong preference patterns, while WK5 allows voters to be indifferent between pairs of alternatives. The probabilities are estimated as the proportion of times no Condorcet winner emerges or the proportion of times the social ordering is intransitive over a large number of simulated elections.

Each program consists of five subprocesses and two loops, one loop embedded inside the other. SC4 and WK5 differ in the assignment of individual preferences only, so they will be discussed together.

Input. The first subprocess simply defines the parameters (m, n, and the number of iterations, x) for the simulation run. Once these parameters have been defined, two loops are set up to simulate the x elections and the choices of the n voters within each election.

Individual Preferences. If we only allow strong preferences (SC4), there are $m!$ possible preference orders. Assuming that each voter is equally likely to have any of these possible preference patterns, we can "discover" each voter's preference pattern over the m alternatives by selecting without replacement according to the uniform probability distribution. Relaxing the assumption of strong preferences, the problem becomes somewhat more complex. For example, instead of 6 transitive orders for 3 alternatives, there are now 13 distinct preference patterns. (There are actually $2^{m-1}m!$ different preference patterns, but many are redundant; for example, $x > y = z$ is equivalent to $x > z = y$.) The logic of individual choice remains the same, however. WK5 determines the admissible profiles, including all strongly and weakly ordered preference patterns and chooses from this set uniformly.

Individual Vote. Each individual's vote must be tallied. This is generated through a pairwise comparison of the m alternatives for each voter, keeping track of the voter's pairwise preferences across all the alternatives. Each individual's "votes" on all pairings of alternatives are stored in a large matrix (VOTE) summing individual pairwise preferences.

Social Choice. Once we have tallied the preferences of all the voters in pairwise comparisons over the m alternatives, a social choice must be made. If the VOTE matrix for the simulated election shows an alternative that is preferred to all others, a CONDORCET counter increments. Similarly, if the matrix shows a completely transitive social order, a TRANSITIVE counter increments. The program then loops to the next simulated election.

Output. After all x elections have been simulated, frequency-based estimates of $Q(m, n)$, $Q^*(m, n)$, $P(m, n)$, and $P^*(m, n)$ are output. These estimates are equal to CONDORCET/x and TRANSITIVE/x, respectively. Following convention, we report $1 - Q(m, n)$, $1 - P(m, n)$, $1 - Q^*(m, n)$, and $1 - P^*(m, n)$.

Notes

The order of authors was determined by the tyranny of the alphabet. We are grateful to Joel Kaji and Jeff Segal for their helpful suggestions. This research was supported by the National Science Foundation under award no. SES-9102901.

1. For an accessible review, see Riker 1982; for a discussion of later developments, see Schofield, Grofman, and Feld 1988.

2. Though it seems clear that Jefferson was influenced by Condorcet, the case for Madison is more tenuous, and whether either truly understood his work on social choice has been questioned (McLean and Urken 1992).

3. Grofman (1993) foreshadows one of our findings when he asserts that social choice theorists greatly overestimate the probability of cycling majorities in mass elections (see also Feld and Grofman 1992).

4. Another questionable assumption of earlier work—that all preference orders are equally likely to be selected by each individual—has not been relaxed in the work reported here, though empirical work also undermines this assumption (Grofman 1993; Niemi and Wright 1987).

5. $P(m, n)$ is a measure of the "robustness" of the social choice process in the sense that it measures the likelihood of attaining a collective order with a majority first, second, . . . , and mth preference over the remaining alternatives. Such a social order contains no ambiguity in the form of cycles or indifferences. For strong individual preferences (containing no indifference), a "strong" collective ordering is assured, though it could be intransitive. When we allow individual indifference, however, collectively transitive orders can contain indifference. So $P(m, n)$ and $P^*(m, n)$ are defined to be the probabilities of getting a fully transitive *and* strongly ordered collective preference order.

6. Indifference relations formally have three properties: reflexive, symmetric, and transitive. (For fuller treatment, see Bonner 1986, chap. 3.) Preference relations are irreflexive, asymmetric, and transitive. We also manipulate the probability of indifference in other simulations reported later.

7. For either the C code or a working copy, compiled for IBM-compatible personal computers, contact Charles Taber, Department of Political Science, State University of New York, Stony Brook, Stony Brook, NY 11794-4392.

8. We have no theoretical explanation for this finding.

9. We think this result adds to the significance of the structure-induced equilibria literature (see Shepsle 1979). This literature emerged as a response to the findings of McKelvey (1976, 1979) showing that in an n-dimensional, majority-rule system, if a majority winner is not produced, the majority-rule relation "breaks down completely" (Ordeshook 1986, 81). Shepsle (1979) demonstrated that institutional arrangements (or structural arrangements) in voting systems such as committee jurisdiction and germaneness of amendments can dilute the pervasiveness of cyclical majorities or prevent them from occurring at all. While we recognize that our probability estimates presented here are not derived in a spatial preference model, we believe that our findings implicitly bear on the importance of structure-induced equilibria. If non-Condorcet winners are much more likely for small groups than for large societies (as is the case when indifference relations are allowed), then the institutional arrangements that preclude cyclical majorities under the traditional assumption of strong preference orderings are extremely important when indifference relations are considered. In short, institutional arrangements in small decision-making bodies (i.e. committees) really matter.

10. Even with today's power, simulating an election with $m = 6$ and $n = 1{,}001$ can take a week of computer time!

11. Notice that $Q^*(3, n)$ approaches .056 for very large n. Oddly, this is the oft-computed value of $Q(3, 3)$. Since we can think of no explanation, we must assume this is a coincidence.

12. Values were also calculated for even n, but for simplicity, they are not reported. In general, $1 - Q(m, n)$ is larger for even than odd n, supporting the work of Kelly (1974). The difference between odd and even electorates, though large for small n, rapidly decreases as n grows large. Kelly's (1986) conjecture that these theorems should extend to the case of weakly ordered individual preferences was also supported, though the difference was extremely small. As we have seen, Kelly's further conjecture that the general behavior of $Q(m, n)$ would be repeated for weak preference orders was not supported.

13. We do not directly manipulate the "amount" of indifference because we do not control how indifferent an individual is. Once an individual is categorized "weak," they are as likely to be $x = y = z$ as $x > y = z$. A different approach would vary the probability of indifference in each binary comparison, which would directly control how indifferent people are likely to be. We prefer our closer analog to the classical analysis, because we treat profiles as the unit for selection.

14. If they are all completely indifferent, however, any election would end in a meaningless tie.

15. Radcliff (1993, 1994) makes a similar point, arguing that ideology constrained the preferences of the electorate in the 1972–84 presidential elections.

16. Note, however, that not many of Arrow's theorems find their mark for strong preferences either. But for strong preferences, the trend monotonically increases with the number of voters, which is Grofman's real point and the major traditional finding that our results amend.

References

Arrow, Kenneth J. 1963. *Social Choice and Individual Values*. 2d ed. New Haven: Yale University Press.

Bonner, John. 1986. *Introduction to the Theory of Social Choice*. Baltimore: Johns Hopkins University Press.

Brady, Henry, and Stephen Ansolabehere. 1989. "The Nature of Utility Functions in Mass Publics." *American Political Science Review* 83:143–63.

Chamberlain, J., J. Cohen, and C. Coombs. 1984. "Social Choice Observed: Five Presidential Elections of the American Psychological Association." *Journal of Politics* 46:479–502.

DeMeyer, Frank, and Charles Plott. 1970. "The Probability of a Cyclical Majority." *Econometrica* 38:345–54.

Farquharson, Robin. 1969. *Theory of Voting*. New Haven: Yale University Press.

Feld, Scott L., and Bernard S. Grofman. 1992. "Who's Afraid of the Big Bad Cycle?" *Journal of Theoretical Politics* 4:231.

Garman, Mark, and Morton Kamien. "The Paradox of Voting: Probability Calculations." *Behavioral Science* 13:306–17.

Grofman, Bernard S. 1993. "Public Choice, Civic Republicanism, and American Politics: Perspectives of a 'Reasonable Choice' Modeler." *Texas Law Review* 71:1541–87.

Grofman, Bernard S., and Scott Feld. 1988. "Rousseau's General Will: A Condorcetian Perspective." *American Political Science Review* 82:567–76.

Kelly, J. S. 1974. "Voting Anomalies: The Number of Voters and the Number of Alternatives." *Econometrica* 42:239–51.

Kelly, J. S. 1986. "Condorcet Winner Proportions." *Social Choice and Welfare* 3:311–14.

Klahr, David. 1966. "A Computer Simulation of the Paradox of Voting." *American Political Science Review* 60:384–90.

McKelvey, Richard D. 1976. "Intransitivities in Multidimensional Voting Models and Some Implications for Agenda Control." *Journal of Economic Theory* 16:472–82.

McKelvey, Richard D. 1979. "General Conditions for Global Intransitivities in Formal Voting Models." *Econometrica* 47: 1085–1112.

McLean, Iain, and Arnold B. Urken. 1992. "Did Jefferson or Madison Understand Condorcet's Theory of Social Choice?" *Public Choice* 73:445–57.

Miller, Gary J., and Thomas H. Hammond. 1989. "Stability and Efficiency in a Separation-of-Powers Constitutional System." In *The Federalist Papers and the New Institutionalism*, ed. Bernard Grofman and Donald Wittman. New York: Agathon.

Niemi, Richard. 1970. "The Occurrence of the Paradox of Voting in University Elections." *Public Choice* 7:91–100.

Niemi, Richard, and Herbert Weisberg. 1968. "A Mathematical Solution for the Probabilities of the Paradox of Voting." *Behavioral Science* 13:317–23.

Niemi, Richard, and J. R. Wright. 1987. "Voting Cycles and the Structure of Individual Preferences." *Social Choice and Welfare* 4:173–83.

Ordeshook, Peter. 1986. *Game Theory and Political Theory*. Cambridge: Cambridge University Press.

Radcliff, Benjamin. 1993. "The Structure of Citizen Preferences." *Journal of Politics* 55:714–19.

Radcliff, Benjamin. 1994. "Collective Preferences in Presidential Elections." *Electoral Studies* 13:50–57.

Riker, William. 1958. "The Paradox of Voting and Congressional Rules for Voting on Amendments." *American Political Science Review* 52:349–66.

Riker, William. 1982. *Liberalism against Populism: A Confrontation between the Theory of Democracy and the Theory of Social Choice*. San Francisco: Freeman.

Schofield, Norman, Bernard Grofman, and Scott Feld. 1988. "The Core and Stability of Group Choice in Spatial Voting Games." *American Political Science Review* 82:195–212.

Shepsle, Kenneth A. 1979. "Institutional Arrangements and Equilibrium in Multidimensional Voting Models." *American Journal of Political Science* 23:27–59.

Taber, Charles S., and Richard Timpone. 1994. "Pictures in the PC: Computational Modeling and Political Psychology." In *New Directions in Political Psychology*, ed. Michael X. Delli-Carpini, Leonie Huddy, and Robert Y. Shapiro. Greenwich, CT: JAI Press.

Young, H. P. 1988. "Condorcet's Theory of Voting." *American Political Science Review* 82:1230–44.

Bradford Jones is Assistant Professor of Political Science, University of Arizona, Tucson, AZ 85721.

Benjamin Radcliff is Assistant Professor of Government, University of Notre Dame, Notre Dame, IN 46556-0368.

Charles Taber and Richard Timpone are Assistant Professors of Political Science, State University of New York at Stony Brook, Stony Brook, NY 11794-4392.

CONTROVERSIES

CIVIC VIRTUE AND SELF-INTEREST

In this Review *in 1993 Shelley Burtt critiqued contemporary republican theorists who urge a reinvigorated citizenry steeped in the ideal of civic virtue. Christopher Duncan finds the essence of Burtt's argument to be that such a revival is not feasible given the level of self-sacrifice required from citizens. He suggests, however, that contemporary republican theorists, like their ancient predecessor Aristotle, are not calling for altruism or the forsaking of self-interest. Rather, virtuous political participation rightly understood is, in fact, among the highest forms of self-interested behavior. Burtt replies that her earlier remarks discussed a part of the republican revival that had little to do with an Aristotelian politics of virtue, which indeed offers an attractive account of political life as a crucial element in individual self-fulfillment. But she notes also that liberalism rightly reminds us of the dangers of public mobilization and concentrated power; moreover, the political and practical obstacles to private reeducation of the citizenry remain.*

COMMENT

Shelley Burtt (1990, 1993) has constructed a very interesting and insightful critique of the politics of virtue as practiced among contemporary theorists of republicanism and communitarianism.[1] Unfortunately—and despite what seem like the best of intentions—her work misses its mark due to a grave misconceptualization and historical misrepresentation of her subject. Even though she gives a passing nod to the most important theorist of virtue, Aristotle, Burtt's presentation of the politics of virtue depends almost entirely on a Roman construction found in the work of thinkers like Cicero, Machiavelli, and Sydney, which, despite her conflating it with its Aristotelian predecessor, is substantively and qualitatively different from the latter. In turn, this misconceptualization, while not wholly debilitating with regard to her own theoretical "proposal," should at least cast doubt about its usefulness once it is viewed in this newly reconfigured contextual light.

Burtt mistakenly writes in the earlier essay that "whether the goal is preserving public liberty or creating an Aristotelian "structure of virtue" for its own sake, the virtue sought after is understood in the same way as this disposition to further public, over private, good in action and deliberation" (1990, 24). I say "mistakenly" because, while Aristotle certainly argues in book 1 of the *Ethics* that the good of the community "is clearly a greater and more perfect thing to achieve and preserve" than the good of the individual, he does not ever suggest that people can be taught to "sacrifice" or "forsake the private for the public" in the name of the state, as Burtt's construction of civic virtue calls for (1993, 364). What Aristotle does suggest is that people are notoriously poor judges of what is in their own best interest and that they need to be educated in virtue in order to achieve their *own* happiness and excellence as human beings. After receiving such instruction in the art of being human, citizens are then supposed to be habituated by the laws and political life to stay the virtuous course. When successful, this process has the beneficial result of generating a well-ordered public sphere superintended by prudent and therefore virtuous citizens. That well-ordered public sphere, then, reciprocally provides the context in which the virtuous citizens can retain their virtue and therefore achieve their own happiness. Man's social nature and the implied "political" context thus render such virtuous public behavior necessary for the private happiness of each individual. In other words, for Aristotle the relationship between civic virtue and self-interest is symbiotic rather than dichotomous. This is an important possibility that Burtt's tension-filled "sacrificial" model of civic virtue either misunderstands or conveniently ignores.

Burtt's understanding of republican civic, or "public," virtue as antithetical and oppositional to modern self-interest is an unfortunate and potentially counterproductive caricature: "Public virtue of any sort asks too great an abstraction from self and the parochial interests that conventionally draw citizens into politics to have any chance of being successfully cultivated in a community that must establish its conditions through the legislative process" (1993, 363). It is precisely our "parochialism" and the "desire to satisfy or protect our personal interests" that necessitates Aristotelian civic virtue (pp. 363–64). As one scholar puts it, Aristotle preached a doctrine of "egoistic eudaimonism," or enlightened self-love (Barnes 1976, 30). Burtt is correct when she argues that "a democratic politics of virtue that transcends self-interest is a seductive but illusory ideal" (1993, 364), but to tell us this is to tell us nothing very important. That people are self-interested is a truism of the first order. Individuals (with the possible exception of a suicide) never set out to harm themselves, or to get the worst of a deal for that matter. Whatever the situation and whatever the value derived, be it intrinsic or material, rational people seek their own good. The interesting question from a theoretical standpoint would seem to be, What good(s) should they seek? The object of virtuous civic participation for Aristotle and many of the contemporary republican theorists cited by Burtt is not to reinvigorate or enshrine the state like Machiavelli and his Roman counterparts but to help individuals fulfill themselves on a multidimensional (and therefore more fully human) rather than unidimensional scale. In other words, it is in the interest of the self, given its social context and "public" dimension, to participate politically and in a virtuous manner.

This is why, while Burtt is right in observing that people "act primarily out of concern for their personal welfare" (1993, 365), her model of Catoian political instrumentalism, or what I would call disturbance-theory civic virtue, is utterly inappropriate. By arguing that "the citizen's role remains primarily that of watch-dog" reacting to danger and alerting "the community to malfeasance" and then pressuring "the government for its correction," Burtt functionally equates the citizen and the spy by her semiconfrontational distancing of the citizen from the polity and vice versa (ibid.) Political life thus becomes the medium by which we do battle, rather than realize our full potential. Once again, the modern self is left alone, desperately afraid, and ever watchful—divided against itself in such a way that even recognizing our own self-interest becomes unbearably problematic and true happiness all but impossible.

Ultimately, it is precisely this reductionist or "private" virtue generated by the politics of liberal individualism with its emphasis on a purely private happiness that is an anathema to self-interest rightly understood as full self-realization. One need not accept the whole Aristotelian teleological system to recognize that what plagues modern men and women is not their lack of a private self-interested existence but, rather, their lack of meaningful connection to other people and the world they share. One hardly needs to promote what amounts to a politics of distrust, suspicion, and fear in the modern "democratic" polity. What modern proponents of republican-based participatory politics seek, for the most part, is to reeducate the modern political mind to see the value and importance for the self of a full political life that it seems to have forgotten somewhere along the way. Burtt's closing contention that "the difficulty with the politics of public virtue in any regime is simply political: no one will vote for it" (1993, 367) thus becomes a cruel non sequitur akin to asking Plato's cave-dwellers why they don't recognize the shadows for what they are and get the heck out of there! Robert Bellah and his associates are therefore absolutely on the mark when they suggest that "what is at issue is not simply whether self-contained individuals might withdraw from the public sphere to pursue purely private ends, but whether such individuals are capable of sustaining either a public or a private life" (1985, 143).

In 1953, Robert Nisbet claimed, "A specter is haunting the modern mind, a specter of insecurity" (1990, 3). He was documenting the loss of community and the onset of political and social anomie. Nisbet, indeed, suggested that modern men and women were in the grips of a pathological condition which has from many indications only gotten worse. For all its promise—and despite our apparent revelry—bourgeois life has proven a gruel too thin to sustain us. In other words, as Aristotle recognized, private life is a necessary, but not a sufficient, condition for a fully human existence. An argument like Burtt's, while trying hard to "take people as they are," fails to take into account what people actually want to—and potentially can—be, even if they themselves may not be fully able to articulate or recognize it yet with some sort of "vote." A "civic virtue" accommodated to her view of self-interest will ultimately help us get what we want, but it won't help us want what we get or to know what we need.

Christopher M. Duncan

Mississippi State University

RESPONSE

Is it possible to publish an account of the politics of virtue that mentions Aristotle only to set him aside? Possible, yes (my work on civic virtue does it), but perhaps neither wise nor prudent. This response to Duncan's note begins by explaining why I relegate Aristotle to the margins of my recent essays (Burtt 1990, 1993). I then accept Duncan's invitation to reflect more thoroughly on the strengths and weaknesses of an explicitly Aristotelian politics of virtue.

In the long history of political thought we find at least two sorts of politics of virtue. The first, most closely identified with Aristotle, makes the cultivation of virtue the end or purpose of politics. As one of its most articulate contemporary spokesmen puts it, "through law in the broadest sense, government must teach virtue directly" (Budziszewski 1994, 38). Not only must government seek to elicit from its citizens their characteristic excellences, but to do so is the government's proper telos, its primary function. Duncan argues in this spirit for a world in which political life is "the medium by which we . . . realize our full potential" (p. 148). It is this sort of polity of virtue that Duncan would like us to take more seriously and that I shall argue is neither possible nor desirable.

But not every political theory that concerns itself with citizen character believes that growing good citizens is the purpose or point of political life. A separate strand of political reflection recommends the civic virtues valued for *instrumental* reasons, regarding them as the crucial guarantor of—even indispensable means to—the advancement of other worthy political ends such as liberty, equality, democracy, and order. I include in this category the Roman writers e.g., Cicero, Sallust, and Livy, later authors deeply influenced by the Roman tradition (e.g., Machiavelli, Harrington, and Rousseau), as well as the contemporary liberal and republican theorists I have discussed at length (Burtt 1993). In principle, an instrumental politics of virtue deserves support, for it seems to me true that the goods we look to our modern nation-states to provide require, for their consistent achievement, citizens habituated to a broad range of civically virtuous actions.[2]

My quarrel, then, is not with the idea of virtuous citizens per se but with those politics of virtue that count on a passionate attachment to political life and the public realm (public spiritedness as traditionally understood) to elicit the desired civic behavior. The problem with this approach is that the sorts of institutional arrangements and educational processes that produce an emotional commitment to the public realm (think here of Rousseau's *Government of Poland*) are largely and thankfully absent in contemporary liberal democracies (Burtt 1993).[3] Our institutions are much better suited to the encouragement of a civic virtue grounded in an enlightened self-interest, along the lines, perhaps, of *Cato's Letters* or recent work by Stephen Elkin (1987) and Bruce Ackerman (1984, 1991).

This whole discussion leaves Aristotle to one side because it is concerned with a tradition of political thought that does the same. I have thus neither misunderstood nor misrepresented the nature of contemporary republican theory. I have simply talked about a part of the republican revival that has little to do with ancient Greek ideas of the good life. Duncan's critique, however, rightly brings back to center stage the question of Aristotle and, with it, the opportunity to address directly the problems of transposing this oldest and most distinguished version of the politics of virtue to a modern setting.

Duncan gives us two reasons to prefer an Aristotelian politics of virtue over both my own suggestions and the instrumental politics of virtue I critique. First, an Aristotelian account of civic virtue links self-interest and the public good in an especially satisfying way. Aristotle never makes the mistake (common to later republicans) of locating civic virtue in "a disinterested attachment to the public good, exclusive and independent of all private and selfish interest" (Wood, 1969, 96).[4] Rather, Aristotle's account of man as a political animal teaches us that "it is in the interest of the self . . . to participate politically and in a virtuous manner" (p. 148). My idea that in certain forms of political action (like the defense of constitutional rights), the pursuit of self-interest grounds civically virtuous behavior, from this point of view, gets the relationship between interest and virtue the wrong way round. In Aristotle's and Duncan's regime, we are first taught to be virtuous by becoming actively involved in the public realm (At whose suggestion? In what way? Is participation optional?) and then discover that this has made us happier, more satisfied human beings.

The second, related reason to prefer an Aristotelian politics of virtue is that it offers both a more credible and a more attractive account of the political life as a crucial element in individual self-fulfillment. As I have described, Aristotle's citizens satisfy their true interests in learning to be virtuous and learn to be virtuous through participation in a well-ordered polity. Political participation in a good constitution is thus a necessary, though not sufficient, part of flourishing as a human being.

One strength of this account is that the liberal objection that individuals may not wish to spend their free time cultivating the public deliberative virtues loses much, if not all, of its normative force. Since individuals will be better persons with habits of public deliberation in place, such participation is in their self-interest. To doubt the wisdom of a strong Aristotelian politics of virtue would thus be irrational. Duncan concludes, "What modern proponents of republican-based participatory politics seek, for the most part, is to reeducate the modern political mind to see the value and importance for the self of a full political life that it seems to have forgotten somewhere along the way" (p. 148).

Given this case for a more explicitly Aristotelian politics of virtue, what can be said for and against it? Duncan speaks enthusiastically of an Aristotelian polity in which citizens receive "instruction in the art of being human" and are then "habituated by the laws and political life to stay the virtuous course" (p. 147). But this idea that we ought to organize our institutions so as to encourage a political participation that will make us more fully human dismisses too quickly two important liberal tenets: (1) the value of assigning to individuals, rather than communities, the right and responsibility to decide what makes for a fully human life and (2) the potential for abuse found in any institution (political or otherwise) that concentrates power, especially in hierarchical ways. Considered from this perspective, the "modern political mind" has not simply forgotten the virtues of a full political life, as Duncan charges; it has reimagined what it means to be a good person so as to escape both the tedium of a full political life and its horrors.

An Aristotelian might reply that it is precisely the liberal polity that has stunted our sense of selves so as not to see the personal rewards of truly engaged and virtuous political participation. But one can accept Duncan's claim that "virtuous political participation is in fact among the highest forms of self-interested behavior rightly understood," while still resisting the idea that the route to such virtue be marked out by the state itself. Even Budziszewski's effort to resuscitate an Aristotelian politics of virtue declares the good government's motto to be, "Protect the true teachers of virtue; get out of their way; and be sure not to make men worse"—specifying, as the true teachers, family and religion (1994, 43). Stephen Macedo (n.d.) defending a far more liberal politics of virtue, has recently reached similar conclusions: the best teachers of liberal virtue will be civic associations, not state legislatures. My point, then, is that even were we to accept Duncan's claim that "virtuous public behavior [is] necessary for the private happiness of each individual" (p. 147), we would want to charge the mediating institutions of civil society (family, religion, and a multitude of large and small civic associations)—not government—with the job of eliciting it.

But grant for the moment the desirability of a tutelary politics of virtue designed to call millions of Americans to a more active political life as a necessary element of "full self-realization." We must still confront the obstinate *politics* of the politics of virtue. What specific steps might neo-Aristotelians take to make their vision a reality? What institutional reforms do they recommend their state legislatures undertake? What curricular changes do they propose to their local boards of education? Political theorists as diverse as Aristotle, Machiavelli, Montesquieu, and Rousseau were refreshingly concrete about the combination of laws, customs, and mores necessary to elicit from small, homogeneous populations a commitment to public virtue (whether also understood to advance self-interest or not). Unless the republican revival is to serve a purely critical purpose, we need similar prescriptions for the large, modern nation-state, along with an acceptable liberal democratic stand-in for the founding legislator these republican theorists believed to be so necessary to the republican project.

It is all very well to speak of "reeducat[ing] the modern political mind," for this locution keeps the object of such reeducation comfortably abstract. But what of ordinary American voters and nonvoters? How are they to be turned to virtue? How will Americans be brought to see, amid the challenges of tedious jobs, limited incomes, family problems, and everyday personal tragedies and triumphs, the value of a full political life in the Aristotelian sense? I have made the case for the impossibility of this task by pointing out both universal human passions (self-love, ambition, desire for material comfort) and foundational features of modern democracies (universal suffrage, party politics, civil liberties) that will frustrate any strong republican schemes for public renewal (Burtt 1993). Those who would defend the possibility (not just the critical utility) of an Aristotelian politics of virtue need to explain first, what resources we might cultivate to overcome these perdurable obstacles and second, whom we might count on to think such reforms would be worthwhile.

SHELLEY BURTT

Yale University

Notes

1. Burtt cites a number of such theorists in her references: Barber 1984; MacIntyre 1981; Michelman 1988; Sunstein 1988; Walzer 1980; Will 1980; et al. Although there are very important differences in the approaches, orientations, and conclusions of these writers, I think the case could be made for including each of them in this rather broad paradigm. Space here does not allow for an extended refutation of specific arguments made by Burtt with regard to particular thinkers, but I would offer the following example as a possible tack one might take. Burtt suggests that Benjamin Barber's position in *Strong Democracy*—"The taste for participation is whetted by participation: democracy breeds democracy" (1984, 265)—is problematic because it "does not reach the problem of getting people to participate in the first place" (1993, 368). Leaving aside the fact that Barber goes to extraordinary lengths in that work to detail specific policy options available to accomplish just such participation, Burtt's theoretical approach here is muddled for other reasons. To name but one, she employs the *fallacy of hasty generalization*—saying in effect, "People are not participating, therefore we cannot get them to participate." Logic aside, however, this non sequitur places the whole critical-theoretical project itself at risk. That project, as defined by Michael Walzer, "is founded on hope; it cannot be carried on without some sense of historical possibility [and] must hold open the possibility of repentance and reform" (1988, 17). Burtt's work implicitly denies such possibilities except in the most incremental or evolutionary of ways. Furthermore, the mode of argumentation Burtt uses seems to ignore the important issue of hegemony in general and liberal hegemony in the United States in particular (Gramsci 1987; Hartz 1954). In other words she does not acknowledge that the behavior of citizens, both active and passive, is the product of complex political conditioning and learning. This necessarily implies that, indeed, "old citizens can learn new tricks." For a larger historical discussion of these issues and questions, see Duncan 1995.

2. Consider for example, Charles Taylor's point that it is difficult to conceive of a free, self-governing polity preserving itself over time if it does not place a high value on citizen participation (Taylor 1989).

3. For an extended argument along similar lines, focusing on American political thought and practice, see Smith 1993.

4. This citation is from the American Revolutionary era. For other examples, see Burtt 1993.

References

Ackerman, Bruce. 1984. "The Storrs Lectures: Discovering the Constitution." *Yale Law Journal* 93:1013–72.

Ackerman, Bruce. 1991. *We the People*. Cambridge: Harvard University Press.

Barber, Benjamin. 1984. *Strong Democracy: Participatory Politics for a New Age*. Berkeley: University of California Press.

Barnes, Jonathan. 1976. Introduction to Aristotle's *Ethics*, trans. J. A. K. Thomson. New York: Penguin Books.

Bellah, Robert N., Richard Madsen, William S. Sullivan, Ann Swindler, and Steven M. Tipton. 1985. *Habits of the Heart: Individualism and Commitment in American Life*. New York: Harper & Row.

Budziszewski, J. 1994. "Politics of Virtues, Government of Knaves." *First Things*, June/July, pp. 38–44.

Burtt, Shelley. 1990. "The Good Citizen's Psyche: On the Psychology of Civic Virtue." *Polity* 23:23–38.

Burtt, Shelley. 1993. "The Politics of Virtue Today: A Critique and a Proposal." *American Political Science Review* 87:360–68.

Duncan, Christopher M. 1995. *The Anti-Federalists and Early American Political Thought*. DeKalb: Northern Illinois University Press.

Elkin, Stephen. 1987. *City and Regime in the American Republic*. Chicago: University of Chicago Press.

Gramsci, Antonio. 1987. *Selections from the Prison Notebooks*. Ed. and Trans. Quintin Hoare and Geoffrey Nowell Smith. New York: International.

Hartz, Louis. 1954. *The Liberal Tradition in America*. New York: Harcourt, Brace, Jovanovich.

Macedo, Stephen. N.d. "Community, Diversity, and Civic Education: Toward a Liberal Political Science of Group Life." *Social Philosophy and Policy*. Forthcoming.

MacIntyre, Alasdair. 1981. *After Virtue: A Study in Moral Theory*. Notre Dame: University of Notre Dame Press.

Michelman, Frank. 1988. "Law's Republic." *Yale Law Journal* 97:1493–1537.

Nisbet, Robert. 1990. *The Quest for Community: A Study in the Ethics of Order and Freedom.* San Francisco: ICS.

Smith, Rogers. 1993. "Beyond Tocqueville, Myrdal, and Hartz. The Multiple Traditions in America." *American Political Science Review* 87:549–66.

Sunstein, Cass R. 1988. "Beyond the Republican Revival." *Yale Law Journal* 97:1539–90.

Taylor, Charles. 1989. "Cross-Purposes: The Liberal–Communitarian Debate." In *Liberalism and the Moral Life,* ed. Nancy L. Rosenblum. Cambridge: Harvard University Press.

Walzer, Michael 1980. *Radical Principles: Reflections of an Unreconstructed Democrat.* New York: Basic Books.

Walzer, Michael. 1988. *The Company of Critics.* New York: Basic Books.

Wood, Gordon. 1969. *The Creation of the American Republic.* New York: Norton.

Will, George 1980. *Statecraft as Soulcraft.* New York: Simon & Schuster.

American Political Science Review Vol. 89, No. 1 March 1995

ARISTOTLE'S SCIENCE OF THE BEST REGIME

In this Controversy, Mary Nichols and Robert Bartlett both enlist Aristotle in support of a liberal politics. Nichols finds that the Aristotle analyzed by Bartlett in his remarks in this Review *in 1994 leads us away from politics into a life of perfect contemplation. The real Aristotle, she avers, by calling attention to the "limits of reason," leaves room for the gods, personal freedom, and shared power. Bartlett responds that the life of contemplative reflection that Aristotle praises is the foundation of knowledge of virtue, without which freedom is license, democracy, dangerous.*

In "Aristotle's Science of the Best Regime," Robert C. Bartlett (1994) analyzes Aristotle's political science in light of the age-old problem of the relation between reason and faith, arguing that Aristotle offers a defense of reason against suprarational or divine claims to guide political life. Such a defense, according to Bartlett, is relevant to liberalism today because its rationalism is challenged not only by modern relativists ("Nietzsche and his successors") but also by the emergence of antiliberal regimes that "appeal to religious truth, especially Islamic law" (p. 143).[1] I am impressed with the urgency of the issues Bartlett raises, as well as his case that Aristotle's political science addresses the claims made on behalf of religious belief. Bartlett demonstrates that "the gods and the divine form the background against which Aristotle sketches his rational science of politics" (p. 144). It is the great virtue of Bartlett's article that it makes us aware of this background, to which scholars have given too little attention.

In spite of Bartlett's claim that Aristotle's political science is relevant to liberalism, however, the concept of reason that he derives from Aristotle directs us away from politics, instead of providing a guide to political practice. I shall suggest, in contrast, that we can find in Aristotle's political science a more practical defense of reason than Bartlett acknowledges and that for Aristotle, reason and piety are in greater harmony than Bartlett's analysis implies.

Bartlett argues that in book 2 of the *Politics*, Aristotle poses philosophy and religion as alternative sources of political life. He points to Aristotle's reference to Onomacritus, who was known for his legislative skill. He was also known for his prophetic art, Aristotle tells us, and for being the companion of Thales, a man whose name is associated with philosophy. By means of this story, Bartlett argues, Aristotle raises the question whether philosophy (Onomacritus' association with Thales) or superhuman inspiration (his art of prophecy) is the source of legislative skill. But while Bartlett presents us with an either–or situation, the story Aristotle tells associates *both* with Onomacritus. It is therefore more plausible that Aristotle uses the story to indicate the dual or complex origins of legislative skill and thus to link politics with both philosophy and piety.

Bartlett is correct in his observation that Aristotle's organization of book 2 as a whole revolves around the issue of philosophy and piety. After discussing three "rational or scientific attempts to delineate the best regime," Aristotle criticizes three existing regimes, which, like all classical cities, are based on religious worship. But Aristotle does not present the regimes of speech or reason as simply preferable to the ones known for their divine origins. Aristotle criticizes both, as Bartlett mentions. But Bartlett fails to draw the inference that regimes based solely on reason are problematic, as well as those that leave everything to the divine. Although he points out that "every virtue of the Cretan regime [that Aristotle mentions] is traceable to its location, i.e., to chance" (p. 146), he does not similarly point out that every defect in the plans of Socrates, Hippodamus, and Phaleas that Aristotle discusses is traceable to a failure to understand the limits of reason. Nor does he mention that in criticizing the *Republic*'s abolition of the family, Aristotle takes the side of piety (1262a25–30).

Rather than infer the limits of reason from Aristotle's criticisms of past thinkers in book 2, Bartlett infers merely the limits of applying reason to politics—and therefore the limits of politics. The limits of reason suggest a legitimate place for piety in human life, including political life. The inability of politics to attain full rationality, in contrast, suggests that politics and the religion to which it is allied in all but liberal regimes are opposed to reason and philosophy. This is the direction of Bartlett's analysis.

The crux of Bartlett's argument for the superiority of Aristotle's rational science of politics to divine law turns on the question of justice. He suggests that Aristotle's political science "earn[s] the right to criticize the divine city above all because it contains a thoroughgoing analysis of the fundamental concern common to both Aristotle the scientist and the inhabitants of the divine city, the concern, namely, for justice" (p. 144). While Aristotle's political science, Bartlett argues, points to the problems inherent in justice, the divine law, blind to these difficulties, promises that justice is attainable. The problems inherent in justice, according to Bartlett, emerge in Aristotle's treatment of the absolute king and, even more important, in his treatment of human responsibility. The problems inherent in justice thus turn on the very principles of equality and freedom, on which liberalism is founded and which the antiliberal and religious regimes that threaten liberalism deny.

In book 3 of the *Politics*, Aristotle discusses various claims to rule. The claim to absolute rule by the individual of outstanding virtue, whom Aristotle calls "a god among human beings," proves the most problematic. Bartlett argues that the absolute king is Aristotle's "political" presentation of the philosopher. While Aristotle cannot make clear in the *Politics* what his outstanding virtue is, due to the limits of politics, he is "as forthright as possible in the *Nicomachean Ethics* 10.7–8: theoretical (contemplative) virtue is superior to moral virtue, and the one possessing the higher virtue to a greater degree is superior to one possessing the lower." The "higher strand of justice," according to Bartlett, which gives to each individual what is due to him, demands that such a godlike individual be given absolute rule in his political community. But his rule will inevitably violate "the lower strand of justice," which seeks the preservation of the community and civic stability, for it is impossible for such an individual to be willingly obeyed by the multitude, who could not "recognize [his] virtue for what it is and thus willingly accept his rule." Because "the possible disparity in virtue among human beings" is "so great," the political community cannot attain justice in the highest sense (pp. 148–49). The divine polity, presumably because it locates gods above humanity and thus fails to recognize gods among human beings, is not aware of this discrepancy and therefore the limits of justice.

The limits of justice in Bartlett's analysis turn not only on the great inequality among human beings but also on their lack of freedom. Indeed, the deepest reason for Bartlett's preference for theoretical virtue over moral virtue is also the reason that he believes that philosophy proves superior to religion. Bartlett points out that the moral virtues and divine law both presuppose human freedom or responsibility. Otherwise, neither the praise and blame we attach to virtue and vice nor divine reward and punishment would be just. But divine law, according to Bartlett, also maintains "that human happiness is attainable on the plane of politics (properly guided and supplemented), or that the problem of justice admits of a political solution" (p. 149). Similarly, the divine law "promises to secure the good of the obedient" (p. 151). In making such promises, divine law must assume that it, rather than the free action of human beings, controls human affairs. The divine law, Bartlett says, "corroborat[es] unawares the philosophic position that denies" human responsibility. Since the divine law also presupposes responsibility when it punishes the unjust, Bartlett concludes, it "contradicts itself" (p. 151).

Wherein, then, lies the superiority of Aristotle's rational science of politics to claims made in behalf of divine law? A rational science of politics avoids the contradiction Bartlett attributes to divine law. One can, of course, avoid a contradiction by denying either side. Given Aristotle's description of the moral virtues and the emphasis he places on deliberative choice in the *Ethics*, one might immediately suppose that Aristotle defends human responsibility. And Bartlett himself mentions that in the *Ethics*, Aristotle refuses to call into question the responsibility of the individual (p. 151). In fact, in the *Ethics*, Bartlett notes, Aristotle rejects the position that an individual's conception of the good (and actions following therefrom) are due to a nature or nurture that he does not control.

Bartlett's attribution of a view of human freedom to Aristotle, however, is qualified. Bartlett claims, for example, only that Aristotle "explicitly" rejects the view that denies human responsibility, leaving open the possibility that he implicitly accepts it (pp. 150–51). And Bartlett gives dignity to the position that denies human freedom—in a way that Aristotle did not—by twice calling it a "philosophic" one (pp. 8–9). He concludes (although "provisionally") that Aristotle's own position is a mean that, while it refuses to question the responsibility of the individual, does question the responsibility of the regime. He refers to the beginning of the *Politics*, where Aristotle says that all communities, including political ones, are constituted for the sake of what appears good to them. Even defective regimes seek the good as they conceive it: "It is therefore more reasonable to conceive of defective regimes as foolish, blind, or mistaken than as unjust or evil; to demand retribution or revenge . . . is strictly speaking irrational" (p. 151). But since Aristotle infers that all communities aim at what appears to them good from what all individuals do, it is difficult to see the basis for the distinction between individual and regime responsibility that Bartlett "provisionally" attributes to Aristotle. Bartlett quotes Aristotle's statement that his explanation of the actions of cities is derived from the actions of individuals (ibid.). Indeed, he even draws attention to what he calls "Aristotle's dictum" that everyone acts for the sake of what seems good (p. 8). There is no reason, then, not to apply Bartlett's conclusion about regimes to individuals as well: the actions of individuals cannot be called unjust or evil, as opposed to foolish, blind, or mistaken, any more than can those of regimes. Laws that punish or reward and community standards that confer praise and blame are therefore "strictly speaking, irrational."

Because Aristotle's political science "culminates in the denial of the possibility of perfect justice on the level of the regime," Bartlett concludes that for Aristotle, "not so much justice and its demands, then, as human nature and its health take pride of place." And the health of human nature resides in theoretical virtue, which has no need of providential gods because it has no need of external support or reward (p. 151). The "thoroughgoing analysis of justice" that indicates the superiority of Aristotle's political science to divine law leads away from justice, as it does from politics. Religion has been thought to turn human beings away from this world to another and to relegate politics to an ultimately unsatisfying activity. Bartlett, in contrast, associates religion with the belief that justice can be realized on earth, with the proper guidance of divine law. Having turned religion into a version of political fanaticism, he allows philosophy to occupy the place often associated with religion. Philosophy, in his view, becomes otherworldly, for it is resigned to the fatalistic view that human actions are determined not by providential gods but by uncaring nature.

In Bartlett's analysis, Aristotle's rational political science is superior to divine law because it understands the limits of justice—limits that stem from the inequality of the best individuals and the irrationality of punishing and rewarding human beings for what they cannot help but do. If Aristotle's political science, however, were not as inegalitarian or pessimistic about freedom as Bartlett suggests, the reasons that Bartlett gives for the conflict between reason and religion become questionable.

Bartlett, for example, fails to note the irony in Aristotle's analysis of the godlike individual. Aristotle compares him to the lion in Antisthenes' fable, who claims to rule because of his superior claws and teeth (*Politics* 1284a15–18). In spite of his boast, however, he is ruled by the same necessities as the rest of the animal kingdom; he is not of an absolutely different order from the ruled. It is to this boastful king of beasts that Aristotle compares the godlike individual who claims absolute rule. He is like a beast in more ways than one, for he destroys the association of free and equal persons that for Aristotle constitutes the political community (p. 148). Aristotle is warning us about the pretensions of individuals who would be gods, not about the pretensions of political communities that try to rule such individuals. It is not the imperfect justice of a politics that cannot accommodate outstanding virtue but the imperfect justice of those who do not accept their similarity to others that follows from Aristotle's analysis. As Bartlett notes, Aristotle says that to divide the offices of the city among this godlike individual and others would be like trying to share power with Zeus (p. 148). But as Homer clearly shows, Zeus's rule was never as absolute as that which Bartlett claims that the godlike individual merits. To say nothing of the various members of the Greek pantheon, whose prerogatives limit Zeus's, Poseidon, Hades, and Zeus actually divided the offices among themselves (*Iliad* 15.185–96). However unequal to others outstanding individuals may be, they are never so unequal that their sharing rule is inappropriate.

Moreover, Aristotle's discussion of the peak of human excellence in the *Ethics* is not quite as "forthright" as Bartlett suggests (p. 148). Aristotle argues there that contemplative activity must be the highest because it is the only activity that could characterize the gods. We could not attribute to them courage or liberality, for example, for what would they fear, and to whom would they give? (*Ethics* 1178b7–15). But Aristotle also argues that conclusions to arguments, such as the one that identifies contemplation with happiness, are unacceptable unless they are confirmed by deeds, by what happens (1179a18–23). He refers to the deeds of gods—good deeds they "do in turn" for those who contemplate, for those who are most like themselves (1179a25–32). But if such deeds contribute to contemplators' happiness, their contemplation is not sufficient for happiness, nor are the gods who so benefit human beings characterized merely by contemplative activity. Deeds that benefit those who deserve benefit appear necessary to divine activity, just as the deeds are necessary to confirm Aristotle's arguments. Aristotle's appeal to the activity of the gods occurs near the end of the *Ethics*, just before the concluding section where Aristotle explains why he must write a *Politics*. Like the gods who do good deeds for those like themselves, Aristotle also turns to the deeds of life—to the various regimes and the ways in which they can be safeguarded and improved.

Deeds could hardly contribute to virtue and happiness—or indeed have any human meaning at all—if they merely followed from forces beyond our control. As Bartlett says, "much (not to say everything) depends on the manner in which the good appears to us," for "human beings cannot but pursue what seems to them good" (p. 150). When Aristotle explores in the *Ethics* the manner in which the good appears, he discusses three causes: (1) our characters or dispositions that we acquire from the repeated actions commanded by our parents and political communities, (2) nature, and (3) ourselves (1114a30–15a3). Aristotle does not definitively resolve the manner in which the good appears, presumably because these different causes play greater or lesser roles for different individuals and at different times. He does, however, clearly maintain that we are "in part causes (*sunaitioi*)" of our characters and hence of our ends (1114b16–24). Something about us other than any nature or nurture that forms our characters also makes us what we are, also makes us in part causes of ourselves. Insofar as we can distinguish ourselves from nature and nurture, we can imagine ourselves other than we are. In such reflection, we stand back to consider what is true and false, good and bad, thereby acquiring the distance necessary for deliberation and choice. Because our deeds come in part from ourselves and not simply from a nature or nurture beyond our control, our deeds as well as our thoughts are a source of happiness, and political communities and divine law can hold human beings responsible.

In Aristotle's view, political life is grounded in this freedom that makes deliberation and choice possible. Politics involves speech about the just and the unjust, the advantageous and the harmful (*Politics* 1253a2–18). It is not surprising that Bartlett, whom I suggested finally opts for "the determined character of the good" (see p. 151), also minimizes the role of reason or speech in political life. He refers to "the brute fact of the persistence of conflicting opinion regarding the good and of the recalcitrance on the part of most human beings to alter or adapt their deepest convictions" (p. 149). But however great Aristotle thought this human resistance to argument, he did view reason as central to politics. And he did base his political science on the possibility of change, urging statesmen to reflect on how they might improve their regimes. When Aristotle criticizes existing regimes, it is not for expecting too much from political life but for leaving too much to chance (see, e.g., p. 146). Bartlett unfortunately pays too little attention to the extent to which Aristotle advises human beings to take control of their political life.

What Bartlett means by "Aristotle's science of politics" in the last analysis is a purely theoretical science. That is, far from attempting to guide statesmen or to help improve political life, Aristotle's science of the best regime teaches us to give up "hope for a 'best regime'" and lets us see that because contemplation is sufficient for human happiness there is "no need to make further demands upon the given world" (p. 152). I therefore find it somewhat disingenuous of Bartlett to conclude that Aristotle's political science is relevant "to the health of liberal democracy" because of Aristotle's "powerful arguments in favor of common decency, responsible self-government, and political moderation" (p. 153). Political moderation emerges from Bartlett's analysis only if political moderation follows from expecting little or nothing of political life. Responsible self-government falls with the position that no regime can be held responsible for its actions. Far from supporting liberal rationalism (as Bartlett at first suggests), his analysis only gives fodder to those antiliberal regimes which deny that "freedom and equality is the organizing principle of political life" and that reason is the "the means appropriate to the discovery of that principle" (p. 143). Bartlett thus misleads us about what we can expect from Aristotle's rational science of politics. I have argued that what Bartlett promised to give us is closer to the truth than what he finally gives us, that Aristotle's science of politics is a practical science that offers an understanding of equality and freedom useful to politics. But Bartlett transforms Aristotle's political science into a theoretical one that leads away from political life, urges an understanding of the great disparity in virtue among human beings, and ignores Aristotle's emphasis on deliberation and choice. It is difficult to see how any of this promotes common decency or contributes to the recovery of a political science helpful to liberalism.

Bartlett does refer us to an aspect of Aristotle's criticism of law that points less to an apolitical philosophy than to a philosophic statesmanship. In favor of proposals for political change, Aristotle refers to the inability of the law to do justice to particular cases because of its necessary universality. As Bartlett notes, the law "must be supplemented by wise judges and interpreters, by human beings who, possessing practical wisdom (*phronesis*), are capable of discerning and adapting the spirit of the law to a given circumstance" (p. 145). The discrepancy between the law and the particular cases for which it was framed indicates, for Bartlett, only "the limitations of law" and therewith the "defective" character of politics (ibid.). Aristotle maintains, however, that statesmen might mediate between law and particulars that arise to achieve a kind of justice that he calls equity (see, e.g., *Ethics* 1137b12–28). Particular cases imperfectly fall under universal law, just as human beings are incompletely constituted by nature and nurture. The gap between particular case and universal law parallels that between a human being and the external influences that form him. The former gap makes statesmanship necessary, the latter makes it possible.

The imperfect rationality of political life does not therefore lead us away from politics to philosophy. Rather, it leads us to a practical reason that corrects, to the extent possible, the unjust consequences that would otherwise follow from the law's generality. I agree with Bartlett, however, that such "defects" in politics can serve as points of entry into philosophy (see p. 145). But the philosophy to which they introduce us is not one that escapes to a life of perfect contemplation. Instead, Aristotle offers a philosophy that arises from and is directed to the imperfections of life in this world. We can love philosophy without ceasing to love our political communities because in their union of reason and unreason, they are not distractions from the "all" sought by both philosophy and piety but its only accessible reflection (see p. 152). If reverence for what eludes human thought and control characterizes piety, rather than either the overt fanaticism or implicit fatalism that Bartlett associates with it, then piety may not be all that distant from philosophy, or even from political life as Aristotle interprets it. And philosophers' pursuit of the truth, however much it might bring them into conflict with the religion of citizens in particular cases, reveals not their radical difference from others but their common humanity. Aristotle's philosophy, which recognizes that we are neither beasts nor gods, is more compatible with piety than the one Bartlett presents as Aristotle's.

MARY P. NICHOLS

Fordham University

RESPONSE

I am grateful to Mary Nichols for the seriousness with which she takes my study of Aristotle's best regime. The most fundamental issue dividing us is the "theological-political problem" and, in particular, its significance for the possibility of the philosophic life; I must limit my reply to a discussion of a few specific points of interpretation, almost all of which are linked in some way with this "age-old problem of the relation between reason and faith" (p. 152), before turning more directly to consider the problem itself.[2]

Although Nichols agrees that the belief in gods and the worship of the divine form the background against which Aristotle's science of politics unfolds, she argues that the conclusion Aristotle means for us to draw in book 2 of the *Politics* is that "regimes based solely on reason are problematic, as well as those that leave everything to the divine" (p. 152). The ambiguity of this formulation aside,[3] it seems to me incautious to conclude on the basis of Aristotle's criticisms of four regimes "based solely on reason" that all such regimes are "problematic." In any event, I did not say that Aristotle holds up these "rational" regimes as models but merely drew attention to the boldness of his criticizing those that claim a divine origin. It seems incautious also to "infer the limits of reason from Aristotle's criticisms of past thinkers in book 2" (p. 152). At the very most, one might infer the limits of the reason of the three thinkers in question. I confess that I cannot quite follow Nichols' assertion of "the limits of reason," limits that "suggest a legitimate place for piety in human life, including political life," for at the same time she criticizes any suggestion of the "limits of applying reason to politics" or of the "inability of politics to attain full rationality" (ibid.). Political life can attain "full rationality," then, despite the "limits of reason" that affect "human life, including political life."

Be that as it may, even if "every defect in the plans of Socrates, Hippodamus, and Phaleas that Aristotle discusses is traceable to a failure to understand the limits of reason" (ibid.), those defects come to be understood as a result of Aristotle's closely argued account of what good politics demands. The defects are judged to be such by the human mind. How little Aristotle defers to the authoritative sources of piety (inspired poetry and divination) is clear from his own best regime, in which he calls for the inclusion of even—or precisely—the priests solely on the basis of an argument meant to be accessible to human beings as such (*Politics* 1328b11–13 and context). As for Aristotle's characterization of outrages committed against mothers and fathers as "impious" (1262a25–29), this need be nothing more than the statement of what used to be, until even very recently, a commonplace. That Aristotle's prohibitions sometimes agree with those of traditional piety does not yet prove that he accepts piety as their ground or justification (cf., e.g., Plato, *Laws* 905a–b and context with Amos 9:1–3). One must not forget that Aristotle fails to include piety in the *Ethics* as one of the virtues belonging to gentlemen.

A further disagreement between us concerns Aristotle's presentation of the problem of justice and, in particular, the significance of the "absolute kingship" with which that presentation concludes. I cannot accept Nichols' suggestion that contrary to what Aristotle says, the political virtues of the truly outstanding individual can never be such that justice itself seems to demand his continuous rule. According to Aristotle, the very claim to merit "ruling and being ruled in turn" is based on an appeal to a given excellence or capacity, and the very partisans who argue on the basis of that excellence, if consistent, would have to yield to someone possessing the same excellence to an extraordinary degree. And yet precisely the statesman of superlative virtue knows his claim to rule is most likely to be met with hostility. Preferring to the satisfaction of this just claim to rule the civil stability that the preservation of the common good requires, such a statesman would in fact cede to the demand to share rule. In practice, then, ruling and being ruled in turn is usually best, that is, safest; it is not always best in the sense of giving to each what most accords with his capacity.

In general, the alacrity with which Nichols asserts "the limits of reason" seems to be balanced by her reluctance to recognize any problem pertaining to justice as it comes to sight in the political community—pertaining, that is, to "political justice" (see *Politics* 1284b16).

If I am not mistaken, Nichols refuses to acknowledge the problem Aristotle delineates in book 3 in part because she contends that Aristotle is a democratic egalitarian. To repeat, Nichols rejects the supposition guiding Aristotle's inquiry into absolute kingship, according to which there are politically significant differences among human beings. She also suggests that I fail to follow Aristotle's thought here because I fail to see his "irony" (p. 154). In my opinion, the interpretation Nichols advances misses the wood for the trees. I cannot do more than make a few brief observations.

First, Aristotle's very definition of "irony," according to which the superior disclaim the estimable qualities they in fact possess, presupposes a difference in capacity and rank among human beings (*Ethics* 1108a22, 1124b30–31, 1127b22–32). It presupposes a meaningful distinction between "the refined" on the one hand and "the many" or "vulgar" on the other—a distinction that Aristotle repeats in his own name throughout the *Politics* and that finds its political expression in the regimes "aristocracy" and "democracy." Indeed, according to Aristotle's famous classification of the regimes, democracy must be placed among the deviant or unjust regimes, with its counterpart among the correct or just regimes, "polity," being only the least good of them: Aristotle explicitly places both aristocracy and monarchy above it (*Politics* 3.7). It would seem that even the democratic partisans whom Aristotle presents are more willing than Nichols to acknowledge the existence of politically significant differences among human beings, for they are inclined to ostracize those in whom they recognize some such superiority (*Politics* 1284a17–b34).[4]

Nichols also suggests that I take insufficient account of the importance of freedom in Aristotle's political science. Without a doubt, Aristotle regards freedom as a good; but freedom apart from the education to excellence or virtue risks being nothing but license, that is, enslavement to evanescent passion. Virtue, then, is more fundamental to Aristotle's science of politics than is freedom.

On the matter of Aristotle's democratism in general, I appeal to the scholar whom Nichols herself might well defer to, Leo Strauss: "I do not believe that the premises . . . democracy is good and Aristotle is good lead validly to the conclusion that Aristotle was a good democrat" (1964a, 93–94). While I wholeheartedly agree that "we can love philosophy without ceasing to love our political communities" (p. 155), it does not follow that any or every philosophy will support the love of any or every community. I myself am inclined to believe that Aristotle would strongly support modern liberal democracy as by far the best of the available alternatives, but we should not hide from ourselves the fact that Aristotle was less concerned with life, liberty, and the pursuit of happiness than with the attainment of true excellence or virtue[5] and that as a result, he thought a healthy political community would have to (as it were) unify church and state (1327a2 and context) and be very small (see 1326b22–24, 1328b11–13, and contexts; cf., e.g., *Federalist* 10). I mean to suggest only that one must proceed very carefully when enlisting Aristotle in the defense of modern liberal democracy: by attempting to transform the *Politics* into the *Two Treatises of Government*, we deprive ourselves of the opportunity to be puzzled, provoked, and even offended by what is new or foreign to us in Aristotle. We thereby deprive ourselves also of the possibility of learning from him.

Concerning Aristotle's understanding of the primacy of the good and its consequences for the rationality of responsibility, my account of it was explicitly—and sincerely—provisional. I am not as clever as Nichols would have one believe. At the same time, if "Aristotle does not definitively resolve the manner in which the good appears" (p. 154), Nichols does attempt to resolve it; but her own account of human action is puzzling to me, amounting to the assertion that "something about us" forms our character and that "our deeds come in part from ourselves" (ibid.). I suspect that such statements are the beginning (though only the beginning) of a satisfactory analysis. As for Nichols' contention that the view opposed to her own is "fatalistic," human actions being in the grip of "uncaring nature" (p. 154), I am uncertain of its precise meaning. If Nichols means by "fatalism" the view that our perception of what is good can never change or improve (i.e., that education is impossible) and by "uncaring nature" that the truly natural human needs, in reference to which genuine goods are good, could never be satisfied by nature or in the natural world, no argument I made or reported supports such conclusions.

Nichols also takes issue with my contention that Aristotle holds the peak of human happiness to consist in the pursuit of theoretical virtue, for she notes both that the wise benefit from divine providence according to Aristotle and that if such providential deeds "contribute to contemplators' happiness, their contemplation is not sufficient for happiness"; "deeds" in general contribute to virtue and happiness (p. 154). But I did not make the mistake of supposing that deeds are irrelevant to the happiness of the wise. Philosophers, too, must eat,[6] to say nothing of the other, higher actions they necessarily perform. The activity of contemplation is—or secures—in the presence of the many goods necessary to life, the most satisfying happiness available to human beings.[7] The health of the soul is of a higher rank than the health of the body, but both are necessary to happiness at its peak.

Nichols is surely right, however, to draw attention to Aristotle's assertion of the providence accompanying the wise man, for it would seem to go against my fundamental contention that the life of philosophic inquiry and the life of pious devotion are essentially different and in fact irreconcilable. Although Nichols does not note the very qualified character of Aristotle's statement,[8] I in no way deny that Aristotle wishes to hold out the possibility that the wise are indeed most beloved by the gods and hence most favored by them. But this very assertion proves to be rather puzzling.

To begin with, it is difficult to see why the gods would favor the wise. As the context of this statement makes clear, the theoretical life is, here and now, the supremely happy one (*Ethics* 10.7–8; see also Plato *Republic* 519c5–6) and is, of all possible lives, the least subject to the vagaries of chance (see *Ethics* 1177a27–b1). Wholly devoted, then, to that activity most needed to secure the happiness within the grasp of human beings, the wise are rather less obviously deserving of divine intervention than are those who undertake acts of outstanding self-sacrifice, for example. Moreover, the "providence" in question cannot be a function of the gods' justice, for Aristotle had argued a page or so earlier that it would be "ridiculous" to attribute to the gods not only courage and liberality, but also justice (for some reason absent from Nichols' paraphrase of the passage on p. 157): "Would they not be manifestly ridiculous making contracts, returning deposits, and all such sorts of things?" (*Ethics* 1178b10–12; see 1179a28 and context). This is not the first time that Aristotle's statements on a matter of some importance contradict (or at any rate are in tension with) one another. In book 1 he had asserted both that it is "altogether absurd" to say that the dead can be happy and that indeed they can be happy (cf. *Ethics* 1100a11–14 with 1101b5–9).

In attempting to wend our way between Aristotle's sometimes conflicting statements, we would be foolish to refuse help wherever available. The surest guide I know to the question of providence in general and to Aristotle's view of it in particular, is Moses Maimonides. Maimonides helps us to see, in the first place, that the view of providence Aristotle states in the *Ethics* is very different from the view he maintains elsewhere in his scientific writings:

> All that, according to what [Aristotle] saw, does not subsist continuously or adhere to a certain order—as, for instance, the circumstances of the individuals of every species of plants, animals, and man—are said by him to exist by chance and not through the governance of one who governs; he means thereby that they are not accompanied by divine providence, and he also holds that it is impossible that providence should accompany these circumstances. (*Guide of the Perplexed* 3.17)

Aristotle's assertion in the *Ethics* that providence accompanies (certain) individuals is an anomaly, then, and perhaps Nichols would agree that Aristotle's final view of providence must be more complicated than she suggests.

Maimonides helps us also to understand the human meaning of the providence mentioned in the *Ethics*, for he himself turns out to accept something very close to it:

> The divine overflow that exists united to the human species, I mean the human intellect, is merely what exists as individual intellects. . . . Now if this is so, it follows necessarily . . . that when any human individual has obtained, because of the disposition of his matter and his training, a greater portion of this overflow than others, providence will of necessity watch more carefully over him than over others—if, that is to say, providence is . . . consequent upon the intellect. Accordingly divine providence does not watch in an equal manner over all the individuals of the human species, but providence is graded as their human perfection is graded. (*Guide of the Perplexed* 3.18)

For Maimonides, as for Aristotle in the *Ethics*, providence is consequent upon the perfection of the intellect rather than that of moral character, and one might well conclude that it *is* the intellect's perfection. I suggest that the "providence" of the philosophic life consists in a specific change in the philosopher's understanding of the world such that he ceases to make those demands upon it that can be satisfied only by gods. I very much doubt whether this view of providence, the one Nichols appeals to, would be satisfying in every respect to the serious citizen—to Job, for example, insofar as he is characterized by "moral virtue and righteousness in action" but "is not said to be a wise or a comprehending or an intelligent man" (*Guide of the Perplexed* 3.22). It is important to see the limited character of the providence accompanying the life of philosophy according to Aristotle—and therewith also the nature of that life in its difference from premodern political life, the peak and completion of which may be said to be the worship of providential gods ordinarily understood.[9] (The difference indicated accords with and may explain the fact, not noted by Nichols, that Aristotle presents the philosophic life as distinct from the political life, between which lives the serious human being must make a choice. See *Politics* 1324a29–33, *Ethics* 1177a28–b26.) According to Nichols, Aristotle's political philosophy as I present it is "otherworldly" and "escapes" to the realm of "perfect contemplation" (pp. 153, 155), whereas I had in fact tried to indicate something of the path one would have to follow, or the questions one would have to confront, in coming to look upon and accept this world, the given world, as the true world.

I turn now to a misunderstanding informing the latter part of Nichols' statement. Because I argued that Aristotle's science of the best regime addresses the theoretical challenge piety poses to the very possibility of philosophy understood as a way of life that bows to no authority other than that of human reason, Nichols concludes that I deny the existence of *phronesis*, or of a practical science of politics capable of guiding statesmen in given circumstances. This conclusion is unwarranted. Apart from the fact, conceded by Nichols (p. 155), that I do indeed give a most important example of such practical guidance, others before me (Nichols not least) have ably brought to light the character of Aristotle's many practical recommendations; and I thought it more important to stress the contribution Aristotle's political science may make toward the resolution of the most fundamental question, that is, the theological-political problem.

Despite Nichols' praise of my attention to "the age-old problem of the relation between reason and faith" (p. 152), she seems not to agree with me either that this problem concerns the very possibility of the philosophic life or even that it really is a *problem*. I contend that if philosophy (including of course political philosophy) rests ultimately on nothing more than commitment or faith, as Nietzsche and his successors have argued is the case, it is distinguished from other commitments or faiths only by its greater pretension, dishonesty, or confusion. Political philosophy at once loses its specific character and becomes indistinguishable from political theology. So far as I can determine, it is some such political theology that Nichols accepts, for philosophy and piety can form the whole Nichols believes they do only if, to repeat, philosophy in the original, full-blooded sense is sacrificed. She comes close to saying it: "Piety may not be all that distant from philosophy" (p. 155). Recall also her quick insistence on "the limits of reason."[10]

In her own way, then, Nichols shares something of the view informing the contemporary crisis of liberalism according to which the foundation originally claimed for liberalism (i.e., autonomous reason as our only star and compass) is an illusion. Nichols may ultimately be correct, but I am less confident than she that the "union of reason and unreason" in politics she unhesitatingly praises will issue only in egalitarianism and the recognition of our common humanity, to say nothing of the possible consequences of her invocation of piety, understood very vaguely indeed as "reverence for what eludes human thought and control" (ibid.). In other words, I do not believe that I, in my attempt to discover a possible grounding of the rational life and therewith a genuine difference between political philosophy and political theology, was in any way "giving fodder" to those concerned with what Nichols calls "fanaticism" (see p. 155).

Nichols is perhaps most critical of my suggestion that Aristotle's science of the best regime may help us to confront the problems facing liberalism today. She goes so far as to suggest that I was being "disingenuous" in making it (p. 155). Let me try to spell out my suggestion. To begin with, a problem must be understood before it can be solved, and I wished above all to highlight the serious obstacles *any* scientific or philosophic understanding of the world faces. Aristotle himself faced the challenge of a politics claiming to be based on divine law. Liberalism is challenged today, not (or not yet) by the external threat of a divinely ordained politics but rather by internal doubts about the very possibility of science or philosophy (i.e., of a simply true grasp of what is). And since liberalism originally claimed to be based on the true understanding of the world achieved by means of a new philosophy, the contemporary crisis of reason is at the same time the crisis of rational liberal politics. I suggested that insofar as Aristotle attempted to found a genuinely scientific political science in full awareness of the genuine obstacles, we may be able to learn from him not only the nature of the problem but also something of its solution, if solution there be. In other words, my study was meant to recover the awareness of the obstacles in question and to call for further study of the ways in which classical political thought may reinvigorate rationalism here and now. I therefore stand by my suggestion, cautious and tentative but in no way disingenuous, that reflection on Aristotle's science of the best regime may contribute to the well-being of liberalism insofar as liberalism wishes to regain for itself a philosophic or rational basis (see, esp., Bartlett 1994, 152–53).

Finally, whereas my presentation of Aristotle's political science leaves intact the two distinct realms of knowledge, practical and theoretical, that come to sight at the surface of his thought, as well as the rank he himself assigns them, Nichols' insistence on the centrality of practical knowledge wholly obscures *theoria* or *sophia*. As Nichols presents him, it is hard to believe Aristotle could have written a *Physics* or *Metaphysics*. The "imperfect rationality of political life"[11] leads only to a certain "practical reason" that "corrects to the extent possible the unjust consequences that would follow from the law's generality"; it leads only to a "philosophy . . . directed to the imperfections of life in this world" (p. 155). Doubtless Pascal exaggerated when he said the following concerning the political philosophy of Aristotle and Plato: "This was the least philosophic and the least serious part of their life; the most philosophic was living simply and tranquilly. They wrote of politics as if they had to bring order into a madhouse" (*Pensées* 331; see also Strauss 1964b, 18). But Pascal is more nearly right than Nichols. He sees more accurately the character of the philosophic life; and in an age in which the nobility and goodness of theoretical philosophy—indeed, its very possibility—have almost been lost sight of, it is better to exaggerate the break Aristotle indicates between philosophy and "philosophic statesmanship" (p. 155) than to have the former collapse into, and so become indistinguishable from, the latter.

The study of Aristotle's political philosophy can awaken us, against the tendency of our times, to the most fundamental question of the best way of life for a human being, as well as to the only serious answers to it: the life of philosophy on the one hand and the life of politics, broadly understood, on the other. But we do not do justice to Aristotle if we obscure the life of simple tranquillity or serene resignation to which he emphatically points and for the justification of which his political philosophy principally exists.

ROBERT C. BARTLETT

Emory University

Notes

1. References to Aristotle's *Nicomachean Ethics* are to the Bywater edition; references to Aristotle's *Politics* are to the Newman edition. Translations from the Greek through p. 155 are Nichols'.

2. I have used Dreizehnter's edition of the *Politics* and Bywater's *Ethics*. Translations hereafter are Bartlett's.

3. The phrase *regimes based solely on reason* could refer either to regimes whose citizens lead fully rational lives, or to regimes based partly on unreason or myth but for rationally defensible reasons (see, e.g., *Politics* 1267b34–35, 1329a27; see also 1336a30 and context). Given Nichols's stress on "the limits of reason," I am inclined to suppose that the "full rationality" available to political life is of the second sort. But this would mean that in this important respect, she agrees with both Plato's Athenian Stranger and Hippodamus.

4. As for Aristotle's allusion to the quip Antisthenes made about the lion and the hares (*Politics* 1284a15–17), of which Nichols makes so much, Aristotle seems to suggest that the lion is as superior to the other animals of the jungle, on account of his far greater strength, as the outstanding individuals are superior to their fellow human beings on account of their far greater virtue. As the lion apparently said to the hares, "Where are your claws and teeth?" (see Aristotle 1984, 255, n. 37), so the men of superlative virtue could say, "Where is your virtue?" To decide whether the lion's claim to be king is as absurd as Nichols suggests, I believe we would need the testimony of a hare sufficiently familiar with the ways of the jungle. (Since the lion does not in fact claim to be above the rule of "the same necessities as the rest of the animal kingdom" or to be of an "absolutely different order from the ruled" [p. 154], these considerations are irrelevant with a view to evaluating his claim.) In addition, whether the absolute rule of Zeus was pleasing to Poseidon does not bear on the question whether Zeus deserved such rule.

5. Cf., e.g., *Politics* 1280a25–81a10 with John Locke's *Letter Concerning Toleration*: "*The commonwealth* seems to me to be a society of men constituted only for the procuring, preserving, and advancing of their own *civil interests. Civil interests* I call life, liberty, health, and indolency of body; and the possession of outward things, such as money, lands, houses, furniture, and the like" (1983, 15; emphasis original).

6. "Both a wise man and a just man, as well as the rest [of the morally virtuous], require the necessities of life" (*Ethics* 1177a28–29). Again, "But [the one who contemplates] will need external well-being, since he is a human being. [Our] nature is not self-sufficient with a view to contemplation; rather, the body must be healthy and food must be available" (1178b33–37). One mark of the philosophic life, however, is that it depends on "deeds"—hence on "equipment"—less than any other life (see, e.g., EN 1178b1–5 and context).

7. Consider, e.g., the following: "If happiness is an activity in accord with virtue, it is reasonable that it will be in accord with the most excellent virtue, and this would be the virtue of what is best. . . . That this is the theoretical [contemplative] has already been said" (*Ethics* 1177a12–13, 17–18). "[Contemplation] is the *only* thing that seems to be cherished for its own sake" (1177b1–2, emphasis mine; see also 1177b19–26). "[Contemplative] life, then, is also the happiest. The life lived in accord with [moral virtue] is happy in a secondary way" (1178a5–9). "Happiness is coextensive with contemplation. The more contemplation is possible, the happier one will be, not accidentally but as a result of contemplation, for contemplation is in itself honorable. Thus happiness would be a certain contemplation" (1178b28–32).

8. "*If* there is some concern for the human things on the part of gods, as *opinion* has it, it would be reasonable for them to rejoice in what is best and what is most akin to them—this would be the mind—and, in cherishing and honoring this above all, they would requite with benefactions those who care for what is dear to them and who act correctly and nobly. It is not unclear that all these things would be available to the wise man above all, for he is most beloved by the gods. It is

likely that this same man is also happiest. As a result, the wise man would thus be especially happy" (*Ethics* 1179a24–32, emphasis mine).

9. Even Aristotle, despite his desire to moderate the influence of the divine in political life, does not go so far as the thinkers of the modern Enlightenment. He is compelled quietly to reintroduce the belief in providential gods in even—or precisely—the best possible form of government (consider *Politics* 1329a27–34, 1330a12–13, 1331a24, 1331b17–18, and book 8 as a whole).

10. Nichols seems to assume that there can be an adequate ground of the philosophic life only if reason is without limits in the sense that it possesses a completed metaphysics or constitutes a comprehensive philosophic system. My essay was meant to investigate the possibility that clarity concerning moral and political matters, supplied not by metaphysics but by political philosophy, may be adequate to provide such a ground (see Bartlett 1994, 153, n. 8).

11. Compare p. 152 of her statement, where Nichols is more reluctant to make such an assertion. Nichols criticizes me both for failing to see "the limits of reason" and for putting too much stress on the limited rationality of politics: I "minimiz[e] the role of reason or speech in political life" (p. 154). But Nichols can equate "speech" with "reason" as she does here only by playing fast and loose with the Greek. From the fact (which I never denied) that human beings possess speech (*logos*), Nichols seems to conclude that everything they say possesses reason (*logos*), or is rational strictly speaking. The very opinions I spoke of would be impossible without the capacity for speech (dogs do not have opinions about justice, nobility, and goodness); but this does not mean that all such opinions are rational. The speech that makes possible the formation of such opinions—and not reason strictly speaking—is "central to politics" according to Aristotle (ibid.).

References

Aristotle. 1970. *Ethica Nicomachea*. Ed. Ingram Bywater. Oxford: Clarendon.

Aristotle. 1970. *Politica*. Ed. Alois Dreizehnter. Munich: Wilhelm Fink.

Aristotle. 1973. *The Politics of Aristotle*. Ed. W. L. Newman. 4 vols. New York: Arno.

Aristotle. 1984. *The Politics*. Trans. Carnes Lord. Chicago: University of Chicago Press.

Bartlett, Robert C. 1994. "Aristotle's Science of the Best Regime." *American Political Science Review* 88:143–55.

Locke, John. 1983. *John Locke on Toleration and the Unity of God*. Ed. Mario Montuori. Amsterdam: Gieben.

Strauss, Leo. 1964a. "The Crisis of Political Philosophy." In *The Predicament of Modern Politics*, ed. Harold J. Spaeth. Detroit: University of Detroit Press.

Strauss, Leo. 1964b. *The City and Man*. Chicago: University of Chicago Press.

NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN JUNE, 1995

The following articles, research notes, and controversies have been tentatively scheduled for publication in a forthcoming issue.

Larry Arnhart. "The New Darwinian Naturalism in Political Theory."

David Austen-Smith. "Campaign Contributions and Access."

Henry Brady, Sidney Verba, and Kay Lehman Schlozman. "Beyond SES: A Resource Model of Political Participation."

Gregory A. Caldeira and James L. Gibson. "The Legitimacy of the Court of Justice in the European Union: Models of Institutional Support."

Dana Chabot. "Thomas Hobbes: Skeptical Moralist."

Daniel Diermeier. "Commitment, Deference, and Legislative Institutions."

David Epstein and Peter Zemsky. "Money Talks: Deterring Quality Challengers in Congressional Elections."

Milton Lodge, Marco R. Steenbergen, and Shawn Brau. "The Responsive Voter: Campaign Information and the Dynamics of Candidate Evaluation."

Karen Orren. "The Primacy of Labor in American Constitutional Development."

Matthew Soberg Shugart. "The Electoral Cycle and Institutional Sources of Divided Presidential Government."

Patricia Springborg. "Mary Astell (1666–1731): Critic of Locke."

Randall D. Lloyd. "Separating Partisanship from Party in Judicial Research: Reapportionment in the U.S. District Courts." A research note.

Laura Stoker and M. Kent Jennings. "Life-Cycle Transitions and Political Participation: The Case of Marriage." A research note.

Robert Lieberman; Helen Ingram and Anne L. Schneider. "Social Constructions and Target Populations (Continued)." A controversy.

ERRATUM

In Richard E. Flathman's review (December 1994, p. 992) of the work edited by Frederick M. Dolan and Thomas L. Dumm, the title should read *Rhetorical Republic: Governing Representations in American Politics.*

BOOK
REVIEWS

American
Political
Science
Review

Book Review Contents

REVIEW ESSAYS

THE CIVIC CULTURE AT 30

Elkins, David J. *Manipulation and Consent: How Voters and Leaders Manage Complexity*
Kornberg, Allan, and Harold D. Clarke. *Citizens and Community: Political Support in a Representative Democracy*
Putnam, Robert D. *Making Democracy Work: Civic Traditions in Modern Italy*
Wilson, Richard W. *Compliance Ideologies: Rethinking Political Culture* . . . . David D. Laitin 168

NEW HORIZONS IN INSTITUTIONAL ANALYSIS

Immergut, Ellen M. *Health Politics: Interests and Institutions in Western Europe*
Knight, Jack. *Institutions and Social Conflict*
Sproule-Jones, Mark. *Governments at Work: Canadian Parliamentary Federalism and Its Public Policy Effects*
Steinmo, Sven, Kathleen Thelen, and Frank Longstreth, eds. *Structuring Politics: Historical Institutionalism in Comparative Analysis*
Weaver, R. Kent, and Bert A. Rockman, eds. *Do Institutions Matter? Government Capabilities in the United States and Abroad* . . . . Elinor Ostrom 174

OPINION AND REPRESENTATION

Gamson, William A. *Talking Politics*
Herbst, Susan. *Numbered Voices: How Opinion Polling Has Shaped American Politics*
Mayer, William G. *The Changing American Mind: How and Why American Public Opinion Changed between 1960 and 1988*
Zaller, John R. *The Nature and Origins of Mass Opinion* . . . . James Stimson 179

POLITICAL THEORY

Grant, Judith. *Fundamental Feminism: Contesting the Core Concepts of Feminist Theory* . . . . Christine Di Stefano 184
Levine, Andrew. *The General Will: Rousseau, Marx, Communism* . . . . Clyde W. Barrow 185

Mann, Patricia S. *Micro-Politics: Agency in a Postfeminist Era* .... Linda Zerilli 185
Melzer, Arthur M., et al., eds. *Technology in the Western Political Tradition* .... Ronald Beiner 187
Ostrom, Elinor, Roy Gardner, and James Walker. *Rules, Games, and Common-Pool Resources* .... Jonathan Bendor 188
Seung, T. K. *Intuition and Construction: The Foundation of Normative Theory* .... George Klosko 189

AMERICAN POLITICS

Aaron, Henry J., et al., eds. *Values and Public Policy*
  Ingram, Helen, and Steven Rathgeb Smith, eds. *Public Policy for Democracy* .... John J. DiIulio, Jr. 191
Anderson, Thornton. *Creating the Constitution: The Convention of 1787 and the First Congress* .... James D. Savage 193
Barkun, Michael. *Religion and the Racist Right: The Origins of the Christian Identity Movement* .... Clyde Wilcox 194
Berk, Gerald. *Alternative Tracks: The Constitution of American Industrial Order, 1865–1917*
  Cortner, Richard C. *The Iron Horse and the Constitution: The Railroads and the Transformation of the Fourteenth Amendment* .... Richard A. Harris 195
Berkman, Michael B. *The State Roots of National Politics: Congress and the Tax Agenda, 1978–1986*
  Roeder, Phillip W. *Public Opinion and Leadership Policy in the American States* .... Paul Brace 197
Bessette, Joseph M. *The Mild Voice of Reason: Deliberative Democracy and American National Government* .... Leroy N. Rieselbach 198
Dodd, Lawrence C., and Calvin Jillson, eds. *The Dynamics of American Politics: Approaches and Interpretations* .... Joel H. Silbey 199
Erikson, Robert S., Gerald C. Wright, and John P. McIver. *Statehouse Democracy* .... Jeff Stonecash 200
Fischer, Frank, and John Forester, eds. *The Argumentative Turn in Policy Analysis and Planning* .... Paul A. Sabatier 201
Ginsberg, Benjamin. *The Fatal Embrace: Jews and the State* .... Lee Sigelman 203
Hart, Roderick P. *Seducing America: How Television Charms the Modern Voter* .... W. Lance Bennett 203
Henig, Jeffrey R. *Rethinking School Choice: Limits of the Market Metaphor* .... John F. Witte 205
Herring, George C. *LBJ and Vietnam: A Different Kind of War* .... David M. Barrett 206
Hinckley, Barbara. *Less Than Meets the Eye: Foreign Policy Making and the Myth of the Assertive Congress* .... Louis Fisher 207
Jones, Charles O. *The Presidency in a Separated System* .... Michael Nelson 208
Kahn, Ronald. *The Supreme Court and Constitutional Theory, 1953—1993* .... Kermit L. Hall 209
Lowry, William R. *The Capacity for Wonder: Preserving National Parks* .... Evan J. Ringquist 210
McCann, Michael W. *Rights at Work: Pay Equity Reform and the Politics of Legal Mobilization* .... Anne N. Costain 211
McKenzie, Evan. *Privatopia: Homeowner Associations and the Rise of Residential Private Government* .... Clarence N. Stone 212
Melnick, R. Shep. *Between the Lines: Interpreting Welfare Rights* .... Jennifer Hochschild 213
Mueller, John. *Policy and Opinion in the Gulf War* .... Robert Y. Shapiro 214
Uslaner, Eric M. *The Decline of Comity in Congress* .... Samuel C. Patterson 215

COMPARATIVE POLITICS

Allinson, Gary, and Yasunori Sone, eds. *Political Dynamics in Contemporary Japan* .... Stephen J. Anderson 217
Anderson, Leslie E. *The Political Economy of the Modern Peasant: Calculation and Community* .... Victor V. Magagna 217
Anderson, Stephen J. *Welfare Policy and Politics in Japan: Beyond the Developmental State*
  Hayao, Kenji. *The Japanese Prime Minister and Public Policy* .... Michael W. Donnelly 218
Baloyra, Enrique A., and James A. Morris, eds. *Conflict and Change in Cuba*
  Mesa-Lago, Carmelo, ed. *Cuba after the Cold War* .... William M. LeoGrande 220

Baylis, Thomas A. *The West and Eastern Europe: Economic Statecraft and Political Change* .... Joel C. Moses 221
Castañeda, Jorge B. *Utopia Unarmed: The Latin American Left After the Cold War*
  Carr, Barry, and Steven Ellner, eds. *The Latin American Left: From the Fall of Allende to Perestroika* .... Jan Knippers Black 222
Cook, Linda J. *The Soviet Social Contract and Why It Failed: Welfare Policy and Workers' Politics from Brezhnev to Yeltsin* .... Philip G. Roeder 223
Crouch, Colin. *Industrial Relations and European State Traditions* .... Philippe C. Schmitter 224
Echeverri-Gent, John. *The State and the Poor: Public Policy and Political Development in India and the United States* .... Norman Uphoff 225
Ferguson, James. *The Anti-Politics Machine: "Development," Depoliticization, and Bureaucratic Power in Lesotho* .... Goran Hyden 226
Haggard, Stephan, et al., eds. *The Politics of Finance in Developing Countries* .... Paulette Kurzer 227
Hillman, Richard S. *Democracy for the Privileged: Crisis and Transition in Venezuela* .... Daniel H. Levine 229
Janoski, Thomas, and Alexander M. Hicks, eds. *The Comparative Political Economy of the Welfare State* .... Charles Tilly 230
Lehman, Howard P. *Indebted Development: Strategic Bargaining and Economic Adjustment in the Third World* .... Nicolas Van de Walle 231
Markovits, Andrei S., and Philip S. Gorski. *The German Left: Red, Green, and Beyond* .... Herbert Kitschelt 232
Morley, Morris H. *Washington, Somoza, and the Sandinistas: State and Regime in U.S. Policy Toward Nicaragua, 1969–1981*
  Spalding, Rose J. *Capitalists and Revolution in Nicaragua: Opposition and Accommodation, 1979–1993* .... Thomas W. Walker 233
Pal, Leslie A. *Interests of State: The Politics of Language, Multiculturalism, and Feminism in Canada* .... Grace Skogstad 234
Rose, Richard. *Lesson-Drawing in Public Policy: A Guide to Learning across Time and Space* .... Lloyd S. Etheredge 235
Searing, Donald D. *Westminster's World: Understanding Political Roles* .... Graham K. Wilson 236
Ward, Alan J. *The Irish Constitutional Tradition: Responsible Government and Modern Ireland, 1782–1992* .... Paul Bew 237
Waterbury, John. *Exposed to Innumerable Delusions: Public Enterprise and State Power in Egypt, India, Mèxico, and Turkey* .... Margaret Levi 238

INTERNATIONAL RELATIONS

Ackerman, Peter, and Christopher Kruegler. *Strategic Nonviolent Conflict: The Dynamics of People Power in the Twentieth Century*
  Wehr, Paul, et al. *Justice without Violence* .... Louis Kriesberg 240
Brawley, Mark R. *Liberal Leadership: Great Powers and their Challengers in Peace and War* .... Raimo Väyrynen 241
Brzoska, Michael, and Frederic S. Pearson. *Arms and Warfare: Escalation, De-escalation, and Negotiation* .... Robert E. Harkavy 242
Carlsnaes, Walter, and Steve Smith, eds. *European Foreign Policy: The EC and Changing Perspectives in Europe* .... David Sylvan 243
Cerny, Philip G., ed. *Finance and World Politics: Markets, Regimes and States in the Post-Hegemonic Era*
  Helleiner, Eric. *States and the Reemergence of Global Finance: From Bretton Woods to the 1990s* .... Virginia Haufler 244
Chafetz, Glenn R. *Gorbachev, Reform, and the Brezhnev Doctrine: Soviet Policy toward Eastern Europe, 1985–1990* .... James Richter 246
Clinton, W. David. *The Two Faces of National Interest* .... J. David Singer 247
Cottam, Martha L. *Images and Intervention: U.S. Policies in Latin America* .... Deborah Welch Larson 248
Featherstone, Kevin, and Roy H. Ginsberg. *The United States and the European Community in the 1990s: Partners in Transition* .... Wolfram F. Hanrieder 249
Forsythe, David P., ed. *Human Rights in the New Europe: Problems and Progress* .... Howard Tolley, Jr. 250
Goldgeier, James M. *Leadership Style and Soviet Foreign Policy: Stalin, Khrushchev, Brezhnev, Gorbachev* .... R. Craig Nation 251

Goldstein, Judith. *Ideas, Interests, and American Trade Policy* .... Henry R. Nau 252
Hagan, Joe D. *Political Opposition and Foreign Policy in Comparative Perspective* .... William J. Dixon 253
Higgins, Rosalyn. *Problems and Process: International Law and How We Use It* .... Richard Falk 254
Keen, David. *The Benefits of Famine: A Political Economy of Famine and Relief in Southwestern Sudan, 1983–1989* .... Barry N. Stein 255
Kupchan, Charles A. *The Vulnerability of Empire* .... Yuen Foong Khong 256
Lundestad, Geir, and Odd Arne Westad, eds. *Beyond the Cold War: New Dimensions in International Relations* .... I. William Zartman 257
Mansfield, Edward D. *Power, Trade, and War* .... Jeffrey A. Hart 258
Nowell, Gregory P. *Mercantile States and the World Oil Cartel, 1900–1939* .... Debora L. Spar 258
Rhodes, Carolyn. *Reciprocity, U.S. Trade Policy, and the GATT Regime* .... Susanne Lohmann 259
Rosati, Jerel A., Joe D. Hagan, and Martin W. Sampson III, eds. *Foreign Policy Restructuring: How Governments Respond to Global Change* .... John A. Vasquez 261
Solingen, Etel, ed. *Scientists and the State: Domestic Structures and the International Context* .... Eugene Skolnikoff 262
Spar, Debora L. *The Cooperative Edge: The Internal Politics of International Cartels* .... Stefanie Ann Lenway 263
Spinardi, Graham. *From Polaris to Trident: The Development of the U.S. Fleet Ballistic Missile Technology* .... Scott D. Sagan 265
Verdier, Daniel. *Democracy and International Trade: Britain, France, and the United States, 1860–1990* .... Wendy L. Hansen 266
Walker, R. B. J. *Inside/Outside: International Relations as Political Theory* .... Jim George 267

American Political Science Review Vol. 89, No. 1 March 1995

THE CIVIC CULTURE AT 30

DAVID D. LAITIN *University of Chicago*

Manipulation and Consent: How Voters and Leaders Manage Complexity. By David J. Elkins. Vancouver: University of British Columbia Press, 1993. 248p. $65.00.

Citizens and Community: Political Support in a Representative Democracy. By Allan Kornberg and Harold D. Clarke. New York: Cambridge University Press, 1992. 285p. $54.95.

Making Democracy Work: Civic Traditions in Modern Italy. By Robert D. Putnam. Princeton: Princeton University Press, 1993. 258p. $24.95.

Compliance Ideologies: Rethinking Political Culture. By Richard W. Wilson. New York: Cambridge University Press, 1992. $44.95.

The field of political culture has been, by Lakatos' criteria (*The Methodology of Scientific Research Programs*, 1978), a "degenerate research program." The books under review give us a sense of why this is so and a model that can guide this research tradition into the "progressive" mode.

This is not to deny that the paradigmatic statement for the political culture field, Almond and Verba's *The Civic Culture* (1963) and the research that followed in its wake have sometimes been bold, innovative, and serious. Indeed, Marxism, Lakatos' ideal-typical degenerate research program, has spawned brilliant and path-breaking studies. *The Civic Culture*, for example, was the first systematic attempt to explain democratic outcomes with cultural variables. Methodologically, it was the first to experiment with comparative survey research, which provided the data for the indicators of the independent variable. The work attracted the attention of generations of scholars who have replicated the findings, criticized the conceptualizations, and refined the theory. In a retrospective volume (*The Civic Culture Revisited*, 1980), Almond and Verba invited strong criticism of all aspects of the research program, some of the most telling coming from the junior author of the original study. (It is also important to note that the implicit dependent variable of much of the empirical parts of *The Civic Culture* was not democracy but democratic participation. This spawned a separate—and quite viable—research program.) All this is evidence of a scientific community at work, with normal science following in the wake of a paradigmatic formulation.

According to Lakatos, however, a progressive research program requires the prediction of novel facts. This would be a difficult criterion for any social science research program to meet; but still, the political culture researchers did not predict the long-term success of democracy in the Federal Republic of Germany, the wave of democratizations in the 1970s and 1980s, or the failures that returned to authoritarian rule. Worse, the political culture researchers could not give plausible accounts of these democratizations and democratic collapses using "political culture" as a principal explanation. Almond and Verba found Italy lacking a civic culture, as did LaPalombara in Pye and Verba's *Political Culture and Political Development* (1965). But Hahn found a city in provincial Russia as having such a culture ("Continuity and Change in Russian Political Culture," 1991). But no practitioner in this tradition would predict either democratic collapse in Italy or democratic stability in Russia. On this first criterion, predicting novel facts, the civic culture program has been a failure.

Crucial also for a successful research program, according to Lakatos, is the reduction of its theoretical claims to mathematics. This allows for the clearest possible specification of the causal mechanisms and for the most rigorous testing of predictions. While again, this may be a difficult burden for most social scientific theory, we can and should demand that our theories have clearly conceptualized variables and well-specified claims about how these variables interrelate. In the civic culture tradition, conceptual clarity was not achieved. Perhaps this is because the authors knew that aggregation of attitudes collected through surveys could hardly predict variance on their dependent variable. Precise specification of mechanisms linking culture to structure, as Lijphart, in *The Civic Culture Revisited*, points out in a judicious review, was lacking.

Links were left ambiguous, and there was little policing by the scientific community to demand sharper specifications. Almond, for example, in his theoretical elaborations, often responded to criticism by adding new variables and including ever more lines of causal influence, rather than specifying more precisely the parameters of a cultural model of politics (in *The Civic Culture Revisited* and Almond, Flanagan, and Mundt's *Crisis, Choice, and Change*, 1973). Also, Almond was perhaps too generous in his support of confirming work that was itself conceptually muddled. In one egregious example, he fulsomely praised a report (Archie Brown and Jack Gray, *Political Culture and Political Change in Communist States* [1977], 254) purporting to show that national cultures outweigh the power of communist parties in influencing political behavior. The cited work, however, did not control for other possible variables. Worse, Almond praised the report in *The Civic Culture Revisited* for showing that political culture "explains the subjective attitude of citizens" (p. 31). Almond ignored the fact that these very attitudes are the indicators of political culture itself. Poorly specified claims and even tautology, without internal policing to root out bad scientific practice, are clear signs of degeneracy in a research program.

Perhaps the worst defeat of the political culture research program was its loss of adherents in the more dynamic parts of the discipline. There are many indicators of this trend. By the 1970s, most comparativists were moving into the field of political economy, almost as a refuge from the stultifying political culture framework. Those who remained interested in the relationship of culture to political institutions found their inspiration in the work of Geertz (e.g., *The Interpretation of Cultures*, 1973), where culture was seen as a set of symbols and rituals that embedded those symbols, rather than a set of attitudes. Culture, Geertz argued, had to be described "thickly" before it could become part of an explanatory heuristic. Brilliant work inspired by such anthropological approaches to culture (e.g., Levine's "Ethiopia," in Pye and Verba's *Political Culture and Political Development*, 1965) could not be incorporated into the civic culture framework and remained uncited in the civic culture literature. Finally, with work on democratization returning to the spotlight in political science, it is significant how little of it relies on "political culture" as a key independent variable. In light of the career choices of the political scientific community, work on the civic culture today has an aura of working out anomalies in Ptolemaic astronomy.

In social science, however, degenerate research programs do not die but only lie dormant until new work, or new conditions, begin to revive it. In the books under review, we can see remnants of the degenerate side of the research program and a paradigmatic statement that gives promise for a progressive problem shift.

An Independent Theory of Culture

Richard Wilson recognizes, in *Compliance Ideologies*, that political culture as an organizing concept had its heyday in political science but has in recent years lost ground to rationalist models, which have turned culture into a residual category (pp. 2–3). His goal is to restore political culture as a concept that can explain both sources of stability in repressive societies (and of repressive elements in modern democracies) and of change to higher levels of freedom and equality.

Wilson's social theory is based on a notion of "compliance ideologies" (reflecting political cultures), which are structures of moral and political reasoning that get ever more sophisticated as societies develop from archaic to traditional to modern and will get more sophisticated still as we move from the modern to the *emergent* (what used to be called, in the 1960s, "consciousness 3"). At each stage of development, there is a trade-off between emphasis on duties to the community (positional criteria) and individual rights based on contract (contractual criteria). He portrays levels of positional and contractual criteria in any society on an x-and-y axis. At each development stage, he draws an indifference curve moving further away from a zero point, where there is no thinking at all about political justification, to higher levels of both positional and contractual criteria.

Individuals, Wilson reasons, seek, for their society, some balance between positional and contractual. Societies—or their elites—wind up at the extremes of these indifference curves and develop compliance ideologies to give merit to the status quo. Change comes about from individuals' pressing for greater balance (i.e., at a 45-degree angle from the zero point) on an indifference curve or, more powerfully, when they develop a higher level of consciousness and seek a structural shift to a more developed indifference curve. At each state the trade-offs between position and contract have opportunity costs; but in the emergent phase, the indifference curve inexplicably becomes convex with the hump at 45 degrees. Consequently, the likelihood of balance between position and constraint is strengthened.

Technically, this book is deeply flawed. The definitions of key concepts change radically throughout the text (and this for a basic concept like political culture; cf. pp. 6, 12, 18, 24, 97, 194). At times, the moral is conflated with the actual, and explanation is often provided in terms of an unreflective functionalism. Also, the stages of development are so reified as to read almost like parody: "In traditional times punishments were severe for infractions of the values cherished by those in authority" (p. 104). Similarly, "In the archaic period ordinary people survived without wealth, significant freedom of movement, or the hope of change" (p. 99). The reversal of Figures 1 and 2 adds to the confusion.

In terms of a political culture research program, a big flaw in this book (like that in the "civic culture" tradition) is that the causal directions are obscure and unconvincing. Consider Wilson's analysis of the transition from the archaic to the traditional stage of development in Greece. Slavery and tyranny, the author claims, were growing in the fourth and third centuries B.C., which led to "a striving for equality" and "a burst of creative activity that, taken together with the rise in Judea of monotheism and the beginning of an elaboration of law, carried the compliance ideologies of the classical world into the traditional phase" (p. 102). Who did the "striving" and "carrying" is left obscure. The relationship of economic incentives, coercive resources, and compliance ideologies is left without specification, though the author asserts a priority to the latter.

A second flaw is that Wilson's notion of culture cannot easily incorporate the insights of scholars who have described actual cultures thickly. Although Wilson places countries in particular historical periods at different points on his indifference curves, I remain unconvinced that his two dimensions (level of development and position/contract trade-off) capture essential cultural differences. Field workers in places Wilson labels as traditional have consistently found mixtures of each phase of development and have continuously refused to code societies along the dimension of archaic to emergent. The most interesting cultural stuff—the symbolic structures embedded in language, religion, and everyday ritual—would find no place on either dimension. *Compliance Ideologies*, in sum, correctly identified the fallowness of the political culture field but does not lead us to greener pastures.

Survey Research and Theories of Democracy

In Kornberg and Clark's *Citizens and Community,* Almond and Verba's research program applying survey techniques to develop a comparative theory of democracy is brought to an extraordinary level of sophistication with scrupulous attention to the details of the data. Extensive surveys in Canada throughout the 1980s permit Kornberg and Clarke to analyze the conditions under which popular support is given to the democratic system and the effects of different levels of support for political compliance.

The principal finding is that political support is not "of a piece" but "multidimensional" (pp. xiv, 32). Citizens do not give undifferentiated support to democracy. Rather, they give different levels of support in increasing levels for political authorities, government (in which they differentially support the judiciary, parliament, and civil service), and the political community. Compliance, their data show, has both an instrumental source (in which the efficacy and fairness of government policies are consequential) and a basis in norms (in which socialization plays a crucial role). Due to a decline in party allegiance, instrumental support has carried a heavy weight in the two-pronged basis of overall support. Consequently, the authors warn, economic swings today can have more ominous effects for the future of democracy than they had in the past.

Kornberg and Clarke are assiduous in taking their readers through every step of their reasoning, and we are therefore treated with some challenging puzzles presented by the data. We get an intriguing discussion on why Canadian citizens attribute economic success to their personal efforts but economic failure to government policies. Another intriguing discussion concerns the phenomenon of decreasing support for the regime found in the course of an election battle, but with the overall effect of an electoral campaign yielding greater popular support (due to postelection feelings of personal efficacy and participation).

The whole of the book, however, is less than the sum of its parts. The world of democracy and its basis in popular support, we are told, is more complicated than we have previously thought. This is undoubtedly true, but the purpose of science is to help us see through worldly complexity, so that we may see our world in a more patterned way. Here the authors fail us.

One reason for this failure is a rather arcane notion of theory. The authors repeatedly invoke "democratic theory" as if it were an object that is "out there." Democratic theory "prescribes" and has "precepts" (pp. 7, 108). With that view of theory, the authors themselves are not obliged to theorize, that is, to move back and forth between deduction and induction in order to give scientific credence to propositions about the role of support in democratic society.

But a more important reason for the problem of the whole being less than the sum of the parts is that the authors have become prisoners of method. In concordance with their use of democratic theory, they *assume* that citizen support is a key element for incumbency and regime health (p. 61). This assumption yields troubling anomalies. Political support appears to be quite volatile, yet democracies such as Canada seem stable. The greatest source of instability is that of regional separation, yet levels of political support coming from those regions do not differ greatly from the rest of the country. The strongest opponents to the regime, furthermore, are not opponents of democracy, so it cannot be determined whether low levels of support reflect attitudes against democracy or against a particular set of state boundaries. Addressing these anomalies with skill, the authors hold to their assumption about support but increase substantially the number of variables needed to achieve statistically significant results.

More than parsimony is lost. Alternate sources of democratic stability—those that have little to do with support—are not systematically considered. Historical data, data on cultural practices, and economic data are presented eclectically, without careful attention to the isolation of variables for theory testing. Most unfortunate is an inadequate treatment of the issue of regional separatism, an issue that concerns the authors both theoretically and empirically. The economic bases for separation are hardly treated. Separation is declared to be "ominous"—for democracy? (p. 259)—yet there is little exposition of the Quebecois point of view and not a single published French-language source in the extensive bibliography!

There are yet other problems with data outside of their survey techniques. The authors make continued reference to the greater role of support for compliance in democracies (as opposed to nondemocracies), but no comparative data are presented to support this claim. And their data on compliance itself are data on attitudes about compliance, not, for example, on actual levels of law breaking. The only data that are taken seriously and treated systematically are data that come from surveys.

For all its methodological sophistication and wealth of data, this book does not give a coherent answer to the question, What maintains modern democracies, and how important is citizen support for that level of maintenance? Instead, we get increasingly complex answers to questions about differential levels of support, with a near explosion in the number of explanatory variables. Kornberg and Clarke's *Citizens and Community* addresses a number of fascinating anomalies on issues of political support, but their solutions are embedded theoretically in a rather unspecified version of political culture. The sum of the findings would be substantially raised if they were linked to a theoretically richer research program.

David Elkins's *Manipulation and Consent* is, like *Citizens and Community,* firmly in the survey tradition. Elkins, however, puts his reader in a time warp. The book itself is a long-delayed response to a debate two decades ago between P. Converse and Robert Lane. Elkins's data are from 1979, and his text refers to work published in the early 1980s as "recent." Despite the timing, the reader quickly sees that this book is both challenging and innovative, with some powerful ideas on the nature of political support in a vibrant democracy.

The principal argument of the book, based on expansive interviews with British Columbian respondents, is that if you ask people to choose issues of greatest importance to them, they will provide complex and sophisticated analyses of those issues, with high levels of internal logic linking problems to possible solutions. While voters may appear ignorant in the standard survey research tradition (where they are asked specific questions about issues of little concern to them), they are far less so if *they* define the issue to be discussed. While few voters have sophisticated syntheses of all major issues, there is a "pluralistic sophistication" in British Columbia that reflects a division of cognitive labor. Elkins reworks Converse's notion of "issue publics" (the manifestation of differential interest in issues by various sectors of the voting public) to demonstrate higher levels of "constraint," or ideological consistency, among the public than Converse thought possible. Using Converse's concepts and commitment to statistical results, Elkins discovered a sophisticated democratic voter closer to Lane's position.

Critics can debate Elkins's findings on the margins. His coding might have been too generous, such as his criterion of *sophistication*. A respondent identified the issue of "energy" as having to do with the lack of it in the general population, and his solution was to develop more sports programs. He received the penultimate score for sophistication for that answer. Perhaps more damaging is that Elkins's theory required him to show lower levels of sophistication on issues that voters did not care so much about. Yet he has no data on issues that voters did not choose to discuss. And as we move down from the most salient issue to the less salient issues among the three chosen by respondents, the level of sophistication, by his coding, goes up. Thus, a division of cognitive labor was not fully demonstrated. Nonetheless, there is compelling evidence in this book (consistent with many recent studies based on survey research not cited by Elkins) that on their own terms, democratic voters are cognitively more competent than early generations of survey researchers realized.

The power of this book is in its theoretical and empirical critique of those survey researchers in the political culture tradition who postulate a necessary aggregate level of citizen support for democracy to succeed. Elkins's model of how democracies work—intense minorities setting up the boundaries of political debate, and office seekers manipulating apathetic majorities within those boundaries set by intense minorities—is far more realistic, though not formally or abstractly delineated. The model shows that the problem of democratic stability is in the political aggregation of issue-sensitive minorities, rather than in some overall level of support for the political community or regime. This is not a new insight. But Elkins's contribution is in the complex interaction between a theory of how democracies work and a data set that gives life to the theory. If aggregate levels of regime support are not crucial for a theory about democratic politics, then collecting such data cannot be justified. Elkins, to his great credit, collected data that speak to a plausible democratic theory.

Elkins's chapter on political culture is equally cogent—and divergent from the Almond and Verba paradigm. He seeks to find within the actual political formulations of his respondents the deeply held axes of agreement, of what is not to be put under question. He finds in British Columbia a "populist" political culture that transcends party and social class. This consensus does not mean an end of ideology or politics, he insists. In fact, a shared political culture allows for intense debates set by the cultural frame—in this case, between those favoring individual responsibility and those favoring collective responsibility. Elkins wisely abjures a general categorization of culture types, as Almond and Verba had with their "civic culture" or as Wilson had with his "emergent stage." What helps sustain democratic life, he suggests, is not the substance of that culture but an agreement that transcends class and locale as to what issue dimensions are salient for political disagreement. Higher levels of trust in government, he finds, coincide with a common "cultural" perspective on what in political life should be taken for granted.

Despite his conjoining theory with survey research and despite his more compelling notion of culture (overcoming two of the big problems I identified in the *Civic Culture* tradition), Elkins does not fully lay out a new research program on culture and democracy. Elkins, indeed, seeks to answer the question of how democracies work (p. 13). But he cannot, by the nature of his study (focusing only on British Columbia) test his theory by looking at the nature of public understanding in countries that are democratic failures. With a focus on only one point of the dependent variable (a successful democracy), Elkins can only show that a cognitively sophisticated public plays an important role in democratic life. But what levels and mixes of the division of cognitive labor (or the degree of cultural consensus needed for democratic stability) cannot be answered within his research design. One way this could have been achieved is by historically tracing how this political culture developed and became institutionalized and how this culture connected with democratic institutions. This would have brought a third dimension (history) into an account that is rich in theory and in survey data. While this book helps to put the survey research and the political culture traditions into a more realistic framework, the relationship of citizen support and political culture to democratic success remains a puzzle.

A Stunning Breakthrough in Political Culture Research

Putnam, acknowledging the long-term collaboration with Robert Leonardi and Raffaella Nanetti, in his

Making Democracy Work sees his research as following the pioneering footsteps of *The Civic Culture* (p. 11). If that were so, I would alter my claim about the nonprogressiveness of that research program. But he is too modest. *Making Democracy Work* is a pioneering work in its own right and has the potential of inaugurating a progressive political culture research program. In this book, Putnam sets up a stunning natural experiment—20 regional governments with a common institutional design created in Italy a quarter-century ago—to ask a simple yet sociologically eternal question: Under what conditions do public institutions serve the public interest? The logic of the argument throughout the book is so clear and the empirical evidence so compelling that any criticism I might give could only be motivated by "that green-eyed monster."

Putnam's argument is that the regional governments throughout Italy, in bringing policymaking institutions closer to the people, changed political behavior in a variety of ways and therefore confirm the views of the new institutionalists who have emphasized the importance of institutional rules to explain political action. But the institutionalization of these governments did not make them equally effective. In fact, there were vast differences in performance of regional governments between the north and south and, somewhat less so, within the north and south. Much of the book seeks to explain that variation. The standard answer is that levels of economic development explain government effectiveness. Putnam first shows that this variable can explain differences *between* north and south but not *among regions within* north and south. More important, his explanatory variable, *civic community* (dense horizontal networks of associations), can explain both sets of differences. And when civic community is used as a control for an economic development model, the ability of economic development to explain variation disappears entirely. Putnam then searches for the source of civic community, and this takes him on an historical journey to the twelfth century, and a *big-bang* explanation: the monarchical solution to the problem of feudal anarchy in the south bred hierarchical patterns of rule and a discouragement of horizontal associations; the republican solution in the north cultivated dense horizontal networks. In the final chapter, Putnam combines "game" and "network" theory to provide a "path-dependent" account (with vicious and virtuous circles) for the persistence of these differences over the centuries. One conclusion for all this is that since civic community explains economic development (rather than the reverse), Mancur Olson's argument that the logic of group formation is to forestall national economic growth is disconfirmed. The deeper conclusion is that democratic institutions cannot be built from the top down (or at least not easily). They must be built up in the everyday traditions of trust and civic virtue among its citizens.

There are three innovations in this book, each meriting comment. First is the methodological innovation within political culture research that I call "triangulation." Second is the theory of democracy. And third is the narrow theory of culture.

Triangulation as the Comparativists' Methodological Principle

Methodologically, Putnam's innovation for political culture research is his triangulation of methodological approaches, using statistics, (semi-)formal theory, and historical (contextual) analysis all woven in a tapestry. Even within the statistical tradition, he seeks confirmation of every point from different angles. He uses survey research to capture public attitudes. However, he does not use it (as did Kornberg and Clarke) as evidence for the success of public institutions. For this, he provides ingeniously developed indicators of government outputs, which stand independent of public attitudes about such outputs. (But in fairness to Kornberg and Clarke, it should be said that Putnam found citizens' evaluations and his own objective indicators to be closely correlated.) Putnam uses game theory to help answer compelling questions raised by empirical findings but goes far beyond many economists in connecting the game theory to empirical results systematically and rigorously realized. And he uses historical and contextual data to weave together a plausible account that is consistent both with the game theory and the statistical findings. The use of a range of methodologies to address a single compelling problem, rather than a rigid commitment to a single methodology, is a major explanation for this book's power. I hope that triangulation of statistical, contextual, and theoretical work within a single study will become standard for future research in comparative politics. Putnam's book shows the impressive results that can then ensue.

Theory of Democracy

Putnam's demonstration that Italy's democratic institutions rest on foundations of civic community is a serious alternative to theories based on social alliances, economic development, and political strategies. No longer can the "civic culture" explanation be treated as a straw man to the anticulturalist alternatives, as it was so often done in the past. Adam Przeworski and others who are finding clear statistical relations between economic growth and the maintenance of democratic institutions will now need to see if their results weaken when a cultural variable is included. Thus, one of the great merits of Putnam's book is that since the theory is well specified, it can serve as a serious contender in theoretical debate about democracy.

But there are two big problems in Putnam's account of democracy that will require some careful thinking before immediate testing. First, his big-bang explanation for civic community, sustained by path dependency, requires criteria to judge what constitutes a big bang. Why was it that institutional design in the twelfth century had longer-term consequences in dividing Italy than Napoleonic rule, political unification, the politics of *trasformismo*, fascism, and the exposure to the post–World War II international political economy had in uniting it? Putnam suggests that the regional governments he has studied are little bangs that can over centuries yield a big bang. But he doesn't specify what is distinctive about the twelfth-century and 1970s bangs that differentiate them from, for example, fascist institutions.

Second, a conceptual problem that runs through the book is the conflation of "democracy" and "effective institutions." Putnam often uses "effective representative institutions" as his dependent variable (e.g., p. 6); but the precise role of "representative" and its relationship to "democracy" in this phrase is never specified, and at times, the relationship between democracy and effective institutions is differentiated only in an ad hoc way (p. 113). To be sure, the institutions he studied were all democratic, so that his focus was properly on effective *democratic* institutions. But perhaps what makes them truly distinctive (as Putnam recognizes) is that they are institutions that spend money that they do not collect, looking more like Soviet firms facing "soft budget constraints" than self-governing political units. It may well be the case that civic community is more important for effective communist institutions than democratic ones. Indeed the historical fact (buried in Putnam's account) that those areas rich in civic community are also those areas that provided principal support for fascism makes one less confident that Putnam has developed a theory of *democratic* institutions. While some careful recalibration of the dependent variable is in order, Putnam has constructed a viable and testable cultural theory.

Theory of Culture

The research program seeking to find the connections among culture, social structure, and political institutions will get new life due to Putnam's book. His thesis is that civic community, based on high levels of political interest, social equality, interpersonal trust (here Putnam relies on the sociological studies of trust by Diego Gambetta), and voluntary association leads to higher probabilities of effective governance and democracy. He gives strong emphasis (though not determination, p. 180) to cultural practices and norms that reinforce civic community even under harsh external conditions that tear apart the fabric of these communities.

Putnam's cultural theory, because it sees cultural practices merely as mechanisms to keep society on its already-chosen path, will receive challenges from "culturalists" who argue that the specific cultural forms that emerge in society have an impact on society and politics far beyond "path-dependent" regulation. For Putnam, the symbolic meanings of rituals are unimportant; for culturalists, they are quintessential. Putnam's compelling accounts of political development in the different regions of Italy, without relying on thick accounts of symbolic activities, poses a challenge to culturalists, more so than Wilson's book, because Putnam accounts for a range of important outcomes with a "narrow" view of culture.

Culturalists will, if they take Putnam seriously, need to show the "value added" by specific information on the content of symbolic practices. They might consider the norms of political corruption exposed in 1990s Italy. The corruption scandals were not treated in Putnam's book, yet they transcend the north-south divide. To explain them properly may require an attention to cultural practices that goes beyond levels of civic community. Here Elkins's content-rich notion of culture is an alternative; but Elkins's theory requires agreement only on symbolic content for democracy to succeed, not on any particular symbolic frame. A richer notion of culture, one built upon the Geertzian framework, might well provide the clue here. But Geertzians will have to specify more precisely than they have in the past what particular forms of culture have what impacts on outcomes (e.g., democracy, levels of corruption, economic growth). This is an important and inviting challenge.

Putnam should also receive challenge from the economists in the "new institutionalist" literature who have developed better-specified models of culture than he has. Avner Greif, for example, defines "cultural beliefs" as the expectations in a society of what other members will do in situations that would occur off the equilibrium path. In "Cultural Beliefs and the Organization of Society" (*Journal of Political Economy*, 1994), he has accounted for the same outcome as Putnam (effective governing institutions in Genoa). But the cultural beliefs that he reports (low levels of trust among members of the community) are precisely the opposite of what Putnam reports for Genoa. Opposite codings on the independent variable with agreement on the coding for the dependent variable invites further research. (Greif's sense of empirical proof, though better than many game theorists, is inadequate for political science. "Triangulation" as a methodological practice is not rewarded in the field of economics, giving political science a comparative advantage in the systematic testing of cultural theories.) In light of new work in economics, it is clear that future work in political culture will need better to specify which aspects of Putnam's civic community drive democratic outcomes. And Putnam's book leaves open the challenge for future scholars to discover the theoretical and empirical connections between trust on the micro level and institutional development on the macro level. Challenges to *Making Democracy Work* coming from the culturalists and the economists should only deepen our understanding of culture, because Putnam has so sharply set the terms of debate.

Political culture research, as many have recognized, was in the professional dog house for a generation. A renewed research program is available to us, and we should begin to guide our own investigations along its lines. The promise is sufficiently great to induce a new generation of scholars to revivify the political culture research program.

American Political Science Review Vol. 89, No. 1 March 1995

NEW HORIZONS IN INSTITUTIONAL ANALYSIS

ELINOR OSTROM *Indiana University*

Health Politics: Interests and Institutions in Western Europe. By Ellen M. Immergut. New York: Cambridge University Press, 1992. 336p. $49.95.

Institutions and Social Conflict. By Jack Knight. New York: Cambridge University Press, 1992. 234p. $59.95 cloth, $17.95 paper.

Governments at Work: Canadian Parliamentary Federalism and Its Public Policy Effects. By Mark Sproule-Jones. Toronto: University of Toronto Press, 1993. 291p. $45.00 cloth, $18.95 paper.

Structuring Politics: Historical Institutionalism in Comparative Analysis. Edited by Sven Steinmo, Kathleen Thelen, and Frank Longstreth. New York: Cambridge University Press, 1992. 257p. $54.95 cloth, $18.95 paper.

Do Institutions Matter? Government Capabilities in the United States and Abroad. Edited by R. Kent Weaver and Bert A. Rockman. Washington: Brookings, 1993. 498p. $36.95 cloth, $16.95 paper.

When Melissa P. Collie, as Book Review Editor for the *American Political Science Review*, asked me to write a book review essay including the above books she wrote that: "As a set, these books address issues relating to the significance of political institutions. They are diverse in their methodologies and all directly deal with the relation between institutions and policy." She hit the nail squarely on the head. All of these books address the significance of political institutions. They relate institutions and policies. Further, they all stress that institutions make an important difference in policy outcomes—jointly challenging the substantial political science literature that has argued that institutions are irrelevant. After the theoretical and empirical onslaught of this set of books, political scientists should affirmatively answer Weaver and Rockman's question—Do Institutions Matter?

While united on these themes, these five volumes are indeed "diverse in their methodologies." The volumes reflect the differences among political scientists in the way that they conceptualize institutions and their consequences. Kathleen Thelen and Sven Steinmo, in their introductory essay to *Structuring Politics*, distinguish between two approaches within "new institutionalism": the historical institutionalist approach and the rational choice institutionalist approach. The major difference they see between these approaches is whether individual preferences are perceived to be endogenous or exogenous. The Weaver and Rockman book represents still a different tradition where institutions are roughly equated with national governmental systems.

Historical institutions, including the chapter authors in the Steinmo, Thelen, and Longstreth volume—Bo Rothstein, Ellen M. Immergut, Peter A. Hall, Colleen A. Dunlavy, Victoria C. Hattam, Margaret Weir, and Desmond S. King—take the goals, strategies, and preferences of actors as something to be explained rather than something to be assumed. Historical analysis is used in this volume to explain why actors seek some goals over others and choose some strategies rather than others in a diverse array of empirical settings including labor markets, health politics, monetary policy, railroad policy, labor union strategies, employment policy, and welfare policy.

Peter Hall's analysis of the dramatic British change from Keynesianism to monetarism that occurred during the 1970s, epitomizes the historical, institutional approach. Hall defines institutions to refer to "the formal rules, compliance procedures, and customary practices that structure the relationships between individuals in the polity and economy" (p. 96). His framework is composed of three levels. At the highest level, he identifies a market economy combined with a constitutional, democratic polity as imposing broad constraints on the direction of policy that could be adopted in Britain and other countries sharing these overarching characteristics. One step down in this framework are features intrinsic to the organization of the political economy of each country including how labor unions are organized, the structure of firms and their associations, the relations among banks within a country and the importance of international economic forces, how the party system is organized, and how administrative agencies at a national and local level are structured. At the bottom of the framework are the standard operating procedures of public and private agencies. The difficulty of changing each of these structural features gets harder as one moves from the regulations and routines of organizational life up through the overarching constitutive structures of a country.

The question that Hall poses is how with all of these multi-level constraints, one could obtain a dramatic change in monetary policy within one decade. The overarching level, while constraining the choice of policy to one that retains markets as key organizing mechanisms for production and distribution, did not narrowly constrain the policies that could be adopted as illustrated by the divergent paths of countries with similar overarching structures. The second and third levels thus were the crucial institutional aspects of Hall's analysis. Labor unions were strong enough to have generated inflationary pressures but fragmented enough to negate efforts toward neocorporatist arrangements.

The intensely competitive, British party system gave the major party that is out of power strong incentives to seek clear policy alternatives to current woes. Further, once a party gained control in a "responsible" party system, it had substantial capacity to inaugurate dramatic change. Rather than thinking of institutions primarily as constraints, Hall urges the reader to recognize that "some kinds of institutional configurations may be systematically biased in favor of change" (p. 107). At the second level, the concentrated structure of the British press spread the word of monetary theory ahead of the Conservative takeover of the national government.

The third level of the historical institutionalists' framework focuses on the operating routines of both government and financial organizations. Here there is a fascinating story of small changes in procedures generating large and at times unintended consequences. As Hall himself notes, many of these changes could be analyzed with game-theoretic techniques that would highlight how the herding instinct of the buyers in an altered bond market gave financial interests a substantially enhanced voice over monetary policy without the need to expend resources in formal organization. A careful reading of Hall's article and several others in this volume illustrates that much of the work in the historical and rational choice approaches to the study of institutions is complementary and not as different as the introductory chapter suggests.

Jack Knight, as an institutional theorist drawing overtly on rational choice foundations, analyzes the processes affecting institutional change—one of the key issues identified by Thelen and Steinmo as the "frontier issues in historical institutionalism" (p. 2). Knight focuses, however, on changes in rules (usually, one at a time) at a microinstitutional level. Knight wishes to explain the processes through which rules become socially shared prescriptions and why one particular rule is adopted rather than others. He challenges earlier efforts to identify socially optimum rules as those that will be adopted. The explanatory factor in the choice of rules, according to Knight, is rather the distributional consequences of the adoption of alternative rules. For Knight, "social institutions are conceived of as a product of the efforts of some to constrain the actions of others with whom they interact" (p. 19, author's emphasis).

In focusing on the bargaining process that implicitly or explicitly is involved in the evolution (or design) of institutions, Knight looks at the differential in the power of participants. A key aspect of that power is the relative threat points of the various participants, that is, the net benefits that would accrue to each participant if there were no agreement in a bargaining situation. If the continuance of the status quo is more beneficial to one participant than to others, and that participant's agreement is needed for a change in rules to occur, then that participant has a stronger position and can differentially affect the outcome of the bargaining over rules.

While the origin of rules for Knight is explained by the relative power position of some participants in relation to others, the maintenance of a system of rules is dependent upon shared understandings of what the rules are and how likely they will be enforced. Thus, rules are changed or sustained depending upon the relative power balance of participants, including the introduction of new participants or changes in the valuation of the resources of prior participants. Rules leading to a suboptimal outcome may be continued in force for long periods of time because the distribution of benefits favors those who would be required to agree to a change.

Knight overtly deals with three of four major criticisms of rational choice theories as applied to institutional analysis and argues that any "theory based on the relationship between rational action and social institutions can readily incorporate questions of historical experience, commonality of experience, or suboptimality" (p. 18). In response to the fourth criticism focused on the assumption of fixed preferences, however, Knight points out that the assumption is difficult to maintain with a historical focus. It is also a difficult assumption to change within a rational choice focus and thus, he continues to assume individuals with fixed preferences throughout the volume. With this exception, Knight bridges the content of both approaches and deals with many of the questions of power and distribution that particularly interest those associated with a more historical approach.

Ellen Immergut's book (as well as her chapter in the Steinmo, Thelen, and Longstreth volume) also bridges both approaches to institutional analysis. By focusing on how interest groups in France, Switzerland, and Sweden pursued their diverse interests and strategies in the political contests over health care policies, Immergut provides an insightful analysis of how institutions—perceived as the rules of the game—affect the capabilities of similar interest groups in differently structured polities. She picked her policy area and cases in order to examine how similarly situated interest groups in regimes where political parties were not strongly divided on the relevant issue ended up with considerably different policies.

Immergut convincingly points out that the numerical or financial strength of the diverse interest groups does not explain the difference in policy outcomes. Rather, the opportunities and constraints created by different institutional configurations at a macro-constitutional level enhance and limit the capabilities of different groups. She convincingly argues that those who affect constitutional structure may do so for the reasons that Knight articulates. She also demonstrates that they may fail in their efforts to predict the array of consequences that follow from their changes. Conservatives who pushed constitutional reforms in Sweden at the turn of the century found that these institutional changes later insulated Social Democratic governments, giving their opponents greater capabilities to initiate national health insurance reforms while providing medical interests with few veto points over these policies.

Veto point analysis not only enables Immergut to explain the difference in policies adopted within the three systems but approaches a general theory that can be applied outside the three systems studied. Veto point analysis is a powerful tool and closely aligned with the type of strategic analysis that emerges from an understanding of games in extensive form. Veto points are essential variables in systems with checks and balances.

By focusing on the rich institutional experience in three countries over time, Immergut introduces major challenges to the presumption that those involved in the choice of constitutional- or collective-choice rules are able to predict how these rules will affect their own positions in the future. Regardless of whether rules are adopted to achieve social optimality or to achieve distributional advantage, the impact of rules—particularly those that affect the structure of diverse arenas—on the incentives of future participants across a long period of time is difficult to predict.

The difficulty in predicting future effects is the result of several factors. First, changes in overarching rules are apt to be long-lasting while the interests of participants change at a more rapid pace. Second, the difficulty of predicting long-term consequences of rule changes is compounded by the creativity of the human actors in inventing new strategies to follow given the opportunities and constraints offered by a change in rules. Hall also points to the new strategies adopted by financial houses and private economic analysts in response to changes introduced by the Heath government in 1971, which were intended only to allow greater competition in the banking sector. Third, rules frequently operate in a configural manner and thus how a change in one rule will affect incentives and behavior over time depends on the particular configuration of other rules that are involved. Given both the large number of single rules that can be changed and the immense variety of configurations of rules, knowledge about the effects of changes in rules over time is particularly difficult to amass. It is not possible, when interaction effects exist among rules, to study changes in one or a few rules in isolation of one another and achieve warrantable knowledge. The problem of misspecifying models is well-understood in the use of econometric models of behavior and outcomes. The problem of misspecifying institutional theories by including rules as simple additive variables has yet to be well-understood.

Mark Sproule-Jones also works at the frontier that Thelen and Steinmo (p. 2) identify by examining institutional change over time and the interaction of institutional and ideational variables in policy formation and change. Sproule-Jones's roots are in both rational choice theory and the historical school of scholars such as John R. Commons. Readers from both traditions will find many important insights here that will affect the way they do their own work in the future.

His book focuses on rule configurations and on multiple layers of rules nested within one another that jointly affect the particular situations in which individuals find themselves in complex societies. Sproule-Jones, like Knight and Immergut, equates institutions with the rules of the game. He stresses, however, that more than one rule tends to operate in most settings and that rules-in-use are developed by participants in both public and private settings. He focuses particularly on rule configurations at a constitutional, institutional, and operational level. Sproule-Jones's three analytical levels are similar to the three levels in Peter Hall's framework. The complexity implied in both approaches is substantial.

Sproule-Jones also distinguishes between rules-in-form and rules-in-use—the latter being the rules followed in a particular time and place to structure relationships around a particular policy process. In order to study the effect of institutions on outcomes, he argues that one needs to learn what the rules-in-use are, that this cannot be done for a macro system as a whole, nor can one learn what rules are in use without substantial empirical research on the ground. Rather, he makes the strong methodological argument that scholars can only understand rules-in-use by intensive field research and not be studying formal legislation and committee reports.

Sproule-Jones overtly introduces the attributes of goods that are of relevance to considering how rules affect incentives and outcomes. If one were to use this aspect of the Sproule-Jones analysis in viewing the articles in the Steinmo, Thelen, and Longstreth volume and the Weaver and Rockman volume, one would try to understand what difference in attributes, such as excludability and divisibility, occurs as one moves from the redistributive policies associated with welfare benefits, to regulating the patterns of relationships between labor unions and management, to structuring the monetary policies of a nation. Rule configurations that generate positive incentives and outcomes when related to the distribution of private goods (excludable and divisible) are likely to generate perverse incentives and counterproductive outcomes when applied to the provision of public goods (nonexcludable and joint). Theoretical accumulation within both the historical and the rational-choice approach would be enhanced by a more self-conscious recognition of the importance of the physical and technological attributes of the policy fields involved in a study.

Sproule-Jones himself focuses on the question of how the nested set of rule configurations of the Canadian federal system affects the capacity of diverse local actors to cope with the development and regulation of a local level water resource: Hamilton Harbor. He looks specifically at commercial shipping (a private good provided largely by cartels of shippers facing a limited set of competing port authorities); pleasure boating (a common-pool resource good obtained by a large number of "squatters" who have no legal property rights to invest in, maintain, or protect their activities); and water quality (a negative externality produced by many different point and nonpoint sources that adversely affect the use of Hamilton Harbor for other potential benefits).

Immergut holds the policy field constant while looking at the impact of diverse institutional arrangements on policy outcomes. Sproule-Jones holds the broader institutional arrangements and local setting constant while looking at three types of local policies bundled together in one resource. Both contribute greatly to our understanding of how asymmetric relationships created by rule configurations can generate relatively predictable outcomes given the attributes of the goods involved.

The multi-author volume edited by R. Kent Weaver and Bert A. Rockman, on the other hand, adopts a still different approach to the study of institutions than either the historical or rational-choice approaches represented by the other four volumes. They turn to a question first posed by Woodrow Wilson concerning the performance of American institutions when contrasted to those of other democracies. They ask the authors of individual chapters in this volume to address three questions:

- What are the effects of difference in institutional arrangements for governmental effectiveness, if any? *Which* institutions matter and how do they affect governmental performance?
- If political institutions do facilitate differences in policymaking capabilities, how do these differences come about?
- How, if at all, can knowledge about institutional consequences be applied? Are differences in institutional effectiveness so severe that reforms are called for in the United States or in other countries (p. 5)?

After reading the other books stressing the incentives of actors within diverse institutions or how rules combine in a configural manner, these highly general questions appear blunt and unanswerable. Further, each case-study author was asked to examine the capabilities of *all* governments included in their study according to ten specific performance measures. The first five of these capabilities are stated as the capacity to:

- *set and maintain* priorities among the many conflict demands made upon them so that they are not overwhelmed and bankrupted;
- *target resources* where they are most effective;
- *innovate* when old policies have failed;
- *coordinate conflicting objectives* into a coherent whole; and
- be able to *impose losses* on powerful groups (p. 6, authors' emphasis).

The hypotheses that they used to initiate their studies were that parliamentary systems, as compared to the U.S. checks-and-balances system, would: (1) have more responsible and accountable governments, (2) have greater capabilities, (3) make superior policy choices, and (4) have a higher probability of turning policy choices into policy outcomes.

Based on a wide diversity of case studies by a distinguished group of scholars, the editors concluded that neither the U.S. nor other industrial democracies had a full set of capabilities as they had defined them and that whether a parliamentary or checks-and-balances system existed was not as crucial as other aspects of the institutional attributes of a system. They reach eight "firm" conclusions that might be characterized by some readers as more vacuous than one would expect given the immense effort of organizing 12 different chapters by a total of 16 major scholars. The first four of these firm conclusions are:

- *Although institutions affect governmental capabilities, their effects are contingent* (p. 446).
- *Specific institutional arrangements often create both opportunities and risks for individual governmental capabilities* (p. 447).
- *Policymaking capabilities may also differ substantially across policy areas within a single political system* (p. 447).
- *Institutional effects on government capabilities are channeled through governmental decisionmaking characteristics* (p. 449).

They also conclude that the institutions of government are better thought of as being composed of multiple layers. After starting out with a traditional focus on governments as a whole, Weaver and Rockman end up where the other volumes in the series begin.

We learn from the entire set of books, and related literature, that we are in an era of remarkable convergence. The recent Nobel prizes in economics to Herbert A. Simon, Ronald H. Coase, James M. Buchanan, Douglass C. North, and Robert W. Fogel, and now to John C. Harsanyi, John F. Nash, and Reinhard Selten, exemplify how much change is going on in a sister social science. All of these Nobel laureates have contributed to how political scientists think about institutions. Coase, North, and Selten, in particular, have explored different questions about how the information available to individuals and the ideas that they use substantially affect behavior and resulting outcomes. We learn from games in extensive form that very small changes in the rules of a game can dramatically alter predicted outcomes. Exactly who moves first, the specific moves allowed, what information is available to which players, the number of times players expect to repeat interactions with one another, the shared cultural norms about appropriate strategies, together with many other precise attributes of a game, affect structure, best response strategies, and predicted outcomes. Experimental work by institutional theorists is slowly generating a better grounded empirical theory drawing on a general framework that includes a major role for human agency but also includes precise control of the particular combination of rules and physical constraints that affect human choice.

While the divisiveness of the behavioral revolution drove us to "separate tables," the emerging congruence among institutional theorists originally coming from diverse backgrounds may finally enable political scientists to get down to the business of building a cumulative discipline. To build that "House of Political Science," many artisans with diverse skills will be needed. Studying micro-level phenomena requires micro-level theories and empirical methods appropriate to testing these theories. Macro-level phenomena also require their own theories and methods of empirical analysis. The reductionist-holist debate has embroiled all of the social sciences, and some of the biological sciences, in a debate that has at times degenerated into name-calling events. Instead of separate tables, we need to imagine ourselves working at separate levels, all viewing a complex mosaic of recursive processes occurring on multiple time-space fields. Those of us working at the lowest level need the help of those who can see broader outlines. Those of us working at a higher level need the help of those who can see how individuals actually interact with one another to produce the higher-level phenomena. In such a scientific house, there is no single level that provides the best answer to all questions. Rather, one has to understand how different levels provide better answers to some questions than others.

OPINION AND REPRESENTATION

JAMES STIMSON *University of Minnesota and Center for Advanced Study in the Behavioral Sciences*

Talking Politics. By William A. Gamson. New York: Cambridge University Press, 1992. 272p. $49.95 cloth, $15.95 paper.

Numbered Voices: How Opinion Polling Has Shaped American Politics. By Susan Herbst. Chicago: University of Chicago Press, 1993. 227p. $24.95.

The Changing American Mind: How and Why American Public Opinion Changed between 1960 and 1988. By William G. Mayer. Ann Arbor: University of Michigan Press, 1992. 505p. $49.50 cloth, $18.95 paper.

The Nature and Origins of Mass Opinion. By John R. Zaller. New York: Cambridge University Press, 1992. 367p. $59.95 cloth, $17.95 paper.

"Public opinion," in modern political science, usually appears before a conjunction. Part of "public opinion and elections," "public opinion and voting," or "public opinion and political behavior," it has been well in the middle of much of the action in American politics for the last three decades. But it has lived at center stage mainly by conjunction, attached as an add-on topic, usually before the "and," to research agendas such as voting behavior which were actually the focus of the real work. The study of what citizens believe and what they prefer, dragged about with its famous companions, has itself been the subject of little investigation. Political scientists, indeed, when they wish to know what Americans believe and what they prefer, are apt to turn to sociology for answers.

This is a curiosity. Quite central to our own conception of political science since at least the behavioral revolution, always central to the conceptions of those outside the field, public opinion has not, in fact, generated much interest, even by those, like myself, who would have described their interests as "public opinion and . . . " Political scientists, asked to characterize what public opinion *is*, are apt to reply by indirection, to speak of the structure of opinion elements, the role of policy preferences in the causal nexus of the vote, the role of opinions in electoral alignments, and so forth. These are important, worthwhile, things to know. But none of them speaks to the simple issue of what opinion is, or the more interesting second level question of how it has changed, or the third level question of how change might be explained. These sorts of questions, very much part of politics, have not until recently been very important to political science.

Opinion, it is widely noted, is a peripheral part of the Michigan voting model, which so dominated early empirical political science. It might have been different. If the perspective of Anthony Downs had motivated the earliest Michigan studies, unlike the social-psychological field theory which did, the studies would have been rich in measures of belief and preference, and the analysis might have honed in on opinion change as a key element. But what we know of the 1950s test-bed of public opinion analysis suggests that such a perspective might have disappointed. In an era dominated by the atomic bomb and fear of foreign enemies, the policy controversies of the time were a poor stimulus for a rich public opinion. Had we seriously focused on opinion, that is, we might well have discredited public opinion studies by failing to find anything of interest. With the study of media influence only recently recovering from the negative findings of the 1940s, one can imagine a similar consensus on the unimportance of policy preferences that would have doomed the study of them for decades. Without the Michigan Model, that is, it might have been different, and worse.

Less often noted, but to my mind more important, is the research design of the voting paradigm. Part of the problem is obvious. The voting studies were immensely successful. That success brought with it a new commitment to survey research and an inadvertent new commitment to cross-sectional design. The survey cross-section design makes the study of macro public opinion silly—one has only a single datum to "study"—and the study of opinion change impossible. Thus early opinion analysts bought into a starting assumption, that "the" interesting explanatory problem was why some individuals thought and behaved differently than others. That operating maxim effectively precluded the study of what opinion was, how it changed, or why.

The first two decades of the voting studies would have nipped in the bud any interest in policy preferences. Policy preference measures were essentially absent in the 1952 study. Incorporated in 1956 were a small number of items which, reflecting the politics of the time, were not very interesting. And they reflected a scope of policy debate probably less rich than witnessed in the 1930s and 1940s, certainly less rich than what came later. Owing to a commitment to a panel design for 1956–58–60, those same items were repeated in the later studies. The ensuing 1962 study was a brave attempt to keep the fledgling series going, but it was a very small scale low-budget affair that had no room for sustaining old policy series or introducing innovative new probes. By 1964, now well into the heady 1960s, the lesson of the flawed 1950s questions learned, a whole new set of policy preference probes was added. These reflected improved question design, a change that had to be made. But the cost was that the 1950s questions were orphaned. Six years later the 1960s questions joined them as the election study developed a third format, designed to permit spatial analyses.

Thus two decades after the voting studies began, a scholar who wished to study policy preferences from them had a very small group of questions asked in 1956, 1958, and 1960 or another group in different format asked either twice or three times in 1964, 1966, and 1988. The first set contains only trivial longitudinal variance, even though it was administered in a four year period that my own work suggests may have been the largest ferment of liberal sentiment in modern times. The second set begins and ends in a period of political turmoil and, with no leverage from use in more normal times, is all but uninterpretable. Thus an important reason for the laggard status of public opinion research is a simple one; there were no useful data. Scholars who, by training and inclination, might have pursued opinion and preference instead devoted their energy and imagination to voting, a task for which the studies were rich, well designed, and, from the miracle of ICPR (now ICPSR), almost universally available. Thus a generation or two of scholars who called themselves analysts of public opinion (but always with a conjunction) did almost no analyses of public opinion. The action was always on the other side of the conjunction.

Commercial surveys were an alternative. But they too weren't particularly promising for many years. Gallup (AIPO) materials have been available for many years in university libraries. But the commercial motivation, selling releases to newspapers, is not well suited to collecting series of citizen preferences on enduring policy conflicts. It works for some. Gallup series on birth control, the death penalty, attitudes toward business and labor are some of the best—and for many periods, the only—available data. But citizen preferences on policy alternatives are generally not hot commercial property. And the Gallup organization had no proprietary interest in them, as it did in its presidential "popularity" or partisanship series. To search through Gallup materials is to rediscover the limits of public interest in politics. Replete with queries about popular culture—for example, "Who is your favorite female singer?"—politics is a more now and then commodity. And beyond the horse race aspect, for which there is commercial demand, commercial polling firms clearly did not need to measure citizen views of the public order to survive and prosper.

The Gallup studies were never available as inexpensive individual level data as the election studies were. One could imagine analyses of the early Michigan items, a time series of length three, because the impoverished time dimension was conjoined to rich cross-sectional information. The longer Gallup series, in contrast, were only time series. Serious public opinion scholarship based on them couldn't happen until the series became very lengthy. Gallup reports were never comprehensive. Many more data were gathered than reported. Scholars could not justify the considerable effort to track down fugitive materials when, having done so, the series were still too thin for serious analysis. Large scale archiving of public opinion materials was vitally important for the purpose of easing the search. The development of the Roper Center made possible the kind of comprehensive search which the value of the materials could not have justified in an earlier era. Before the Roper Center (and, in practical terms, before its materials became available on-line), no rich knowledge of public opinion could have been based on the product of the commercial survey firms.

The National Election Studies, otherwise a treasure of research opportunities, are still an impoverished resource for public opinion research. And they will always remain so. The problem lies with elections, not election studies. Once every two years isn't good timing for the study of public opinion and its changes. We know from huge literatures on issues like presidential approval that much that is interesting about opinion change needs to be studied on a fine time scale. Much of that movement is studied with quarterly or monthly data. Some recent work on opinion movements around the Gulf War finds it necessary at critical times to get down to the daily level. And the election series is not particularly suitable either for the secular movements of opinion highlighted in Mayer's style of work. If opinion moves in five, ten, or twenty year lurches and cycles, the election studies offer too few biennial readings of too few issues to track the changes. Serious progress in the study of policy preferences could not begin while political scientists were tied solely to "their own" election study series as a data source.

So what? Why should we care? In a representative democracy there is some prospect—no sure thing—that public opinion matters for the formation of public policy. Take that away and the study of opinion becomes as interesting as cataloguing buttons. Representation, that is, is the justification for the study of public opinion, the only one that matters. It is finding the Holy Grail, demonstrating the process of representation, that makes the otherwise tedious collection and analysis of survey results come alive with meaning. We have not come close to achieving this objective, part of its guile. If it were easy, and we had seen it, face to face, it might, like Oz, be ordinary and lose its appeal. But we haven't yet come close enough to fear disappointment.

We now have a good deal of cross-sectional evidence of representation. On quite a number of issues it is safe to say that areas which tend to have liberal opinions tend to be represented in Congress by liberals, conservative areas by conservatives. These relationships, in econometric terms, fit reduced form models. They tell us that X, public opinion, comes to be associated with Y, representative attitudes and behavior, and the evidence is clear enough that it is not widely repeated any more. But the critical questions about representation are issues of cause and process, that which intervenes between X and Y, and these are unidentified in a cross section. We can tell any story we wish about representation mechanisms and predict the same sorts of findings. The problem is that we can't distinguish between the stories. They all (accurately) predict the same findings. Sometimes this is not a big problem. Here it is. The reduced form relationships carry little surprise, and hence little information.

Our intuitions about representation are about process. They are not so much about whether it works as how. That liberal areas tend to be represented by liberals is an impoverished kind of representation. Our most primitive ideas about representation have to do with opinion change. We wish to know whether when opinion changes—and all these books tell us that it does—government responds. When a district, or a state, or a nation becomes more liberal, do its representatives follow? If they do, and perhaps also if they do not, then explaining why gets us right into the gut of politics.

In our texts public opinion is a chapter or two. The various branches of government are usually a chapter each. And the connection between what the public wants and what the government does is on the page fold between them. Public opinion is conceptualized as a set of measures and processes that do not speak to government. Governing institutions are studied in a manner which doesn't deny public opinion influence, but doesn't permit its active study. When citizens of Washington, D.C. could not vote the analogy was complete; all opinion was outside the beltway, all government was inside.

Scholars of government tend to believe in the influence of public opinion on President, Congress, and often even Court. Scholars of public opinion must believe it. But like an ill-coached relay team, we never quite pass the baton. The coaching, in my view, is mainly needed for the opinion leg of this race. So long as it occupies itself with issues other than "what the public believes and what it wants from government," public opinion scholarship has nothing to pass on to those who pursue government in Washington. But it is now possible to pursue those issues, and progress is starting to be made on them. This is really a very new kind of study, one that is public opinion itself, not public opinion with conjunction.

Mayer's *The Changing American Mind*, of these four books, most closely addresses the issues of what the public believes and what it wants from government. If Mayer doesn't reach beyond the page fold and into government itself, he at least can be said to pass the baton smoothly to the next leg. This volume, except for its more recent publication, would properly have been reviewed as part of a set of recent books by Page and Shapiro and by myself. The three books have in common taking seriously exhaustive collections of survey marginals as a means of addressing change in national public opinion over time. These macro level studies have the particular advantage of permitting the analyst to relate changes in belief and preference to other aspects of American politics, of breaking free from public opinion as survey research.

Mayer's early chapters are a description of the contours of public opinion—mainly of policy preferences, that part of most interest to political science—as we know it from the complete survey research record. "Descriptive" is often an epithet among scholars. But it does help to know which facts are in need of explanation before one builds a theory. Mayer argues, and I agree, that political scientists are not very closely in touch with the reality of public opinion in America. Thus Mayer takes us through it, area by area, issue by issue, and with appendices that are a virtual public opinion archive. His is a refreshing straight-down-the-middle approach, not building opinion movements out of sampling fluctuations, not disparaging real movements when they occur. To read this steely objectivity about matters that arouse us is to recognize how rare is straight treatment of these issues.

Objective description of the contours of opinion and opinion change in hand, Mayer then takes us on a tour of the leading possibilities for explaining change in most of the rest of the book. Separate chapters explore population replacement, demographic shift, external events, and media effects as plausible candidates. These analyses cannot be very systematic, for the data do not permit much systematic analysis, but within those limits, there is still much to be learned. We learn, for example, that the racial and gender equality trends are driven more by population turnover than by conversion, but that both processes are clearly at work. External events loom largest in some kinds of foreign and defense policies. Mayer's final chapter, which might be entitled "Why Liberals and Democrats Lose," is a summary essay on opinion and issues as electoral strategy. Mayer sees liberal failure not as a rejection of its New Deal platform, which remains immensely popular, but from shift of emphasis to cultural issues (demanded by the Democrats' important and growing base of upper middle-class activists). These cultural issues, race an important one, have the property that the liberal position was never a popular one.

William Gamson, in *Talking Politics*, is interested in how ordinary people think about politics. The emphasis is on how, on turning information inputs into coherent thoughts. And he is particularly interested in a small class of thoughts that produce action, collective action frames. Although Gamson's "frames" bear a strong resemblance to Zaller's "considerations," the style and focus of this research program is strikingly different from the norm of public opinion studies.

Gamson recruits subjects from a range of ordinary backgrounds and occupations, collectively "working people," to come together (typically in a living room) for a discussion of political issues. Supplied with standard topics, four predefined issue conflicts, and inputs from facilitator scripts, media content, cartoons, and so forth, the focus group participants are set off to talk with only mild steering of the discussions. The conversations that emerge are the data of the book. Faced with public policy problems, whether government should aid a troubled industry, for example, participants talk it through. That produces some quantitative indicators of the development (or more often nondevelopment) of collective action frames, how often issues are conceptualized as matters of injustice, whether framed in a "we" vs. "they" context, for example. Much of the analysis is more impressionistic, with both author and reader encountering verbatim scraps of conversation to make sense of meaning.

This approach has the great heuristic strength of focus groups. There is a richness to these conversations that is very different from the question and answer dialogue of the survey, very much more like real political conversations. And it has the weakness of that approach as well. Rich in impressions, emersed in the real discourse of ordinary people, it is also very hard to stand back and make scholarly sense of what has occurred. One finishes impressed with the ability of participants to relate everyday experience to political life as experienced through the mass media.

Susan Herbst's *Numbered Voices* is about counting in politics and in journalism about politics. This is a work mainly of history. Its perspective is that quantification is the handmaiden of the rationalization of social and political life. Its journalistic treatment of public opinion is in many ways a contrast to the three works of and by political scientists. Its view of public opinion focuses on how it is measured and reported, not what it is. It is more about "polls" than opinion.

One will find here topics well beyond the range of political science. Where we have fastened upon survey research as our almost exclusive window in the views of ordinary people, Herbst's volume treats matters such as straw polls and the estimation of crowd sizes at almost equal length to surveys. That reflects, of course, an historical span in which survey research is a decided newcomer. There is much richness of historical detail in the treatment of these alternate opinion technologies. The substance of Herbst's treatment is much in contrast to political science too. Here one will find focus on the horse race pre-election polls, a matter which is uncommon in the modern opinion scholarship of political scientists.

Reading this work about public opinion measurement in journalism is instructive for academic political science. Its focus on the technology of knowing public opinion reads much like what was written about the topic by political scientists in the first two or three decades of public opinion scholarship. To read that early work is to see a discipline fascinated with the technology of polls and surveys. To be a public opinion scholar then almost automatically entailed professional focus on the "how" of surveys. Political scientists then took intense interest in matters such as sampling, supervision and training of field staff, instrument design and so forth. We saw ourselves as potential producers of survey research, even though that potential was only rarely realized. The success of the election studies and of data archiving turned us into consumers. Something is lost in the transition, but something is gained too. Becoming consumers of data produced by professional survey organizations has freed us to concentrate on data analysis, which is what we do best. The blossoming of opinion scholarship in the 1990s reflects that freedom. Not in control of the data gathering process; not, for example, tinkering with question wording may have freed us to think about what survey responses mean, leading to the insights of the Zaller book.

John Zaller's *The Nature and Origins of Mass Opinion* is left for last for a purpose; it needs to be singled out. This is perhaps the best book ever written about public opinion. It starts with elegant encompassing theory and goes on to make sense of everything we know, including numbers of stray—thought-to-be-unrelated—findings that all blend together into a coherent whole. Zaller starts with a number of tidbits of theory and observation, most importantly, his own (with Stanley Feldman) theory of the survey response. He puts them together, and what comes out is considerably more than the sum of the parts. It is a whole new view, from the bottom up, of what public opinion is.

We began the empirical study of public opinion with the idea that policy issues posed tradeoffs between competing values and priorities—considerations, in Zaller's language—and that the survey response was the citizen report of where he or she had come out after working through the issues. It was, that is, a report of a fixed position. For each issue citizens were arrayed along a spectrum of possible positions, let's say left to right for convenience, and the task of survey research was simply to measure those positions.

The citizens we confronted did not look much like that, but they did mostly answer the questions and, albeit with low reliability, we were able to measure positions. For some citizens reliability seemed to be essentially zero on most issues, and so the original notion had to be modified to include the prospect that some "reports" were fabricated on the spot from respondents who had no real attitudes. This now familiar "nonattitudes" position casts a little doubt on the sanctity of the survey report. But it also salvages the fixed position idea. If respondents seem to be wandering about over time, it is really just that we are aggregating those with truly fixed positions with those wandering aimlessly.

The Zaller position, at once only a minor variation and a radical innovation, is that most respondents do not have fixed positions in the first place. They have real attitudes, but not fully formed fixed ones. Those real attitudes are the result of consulting considerations to arrive at conclusions. But the drawing upon considerations is partial, most likely to draw those at the "top of the head," and thus it has some stochastic properties. Several considerations could have been relevant to the response, but only one or a few are consulted in real circumstances. The fixed position story is static; we believe we are observing the result of some consideration balancing process that occurred before we came on the scene and which has reached a final result. The Zaller attitude, in contrast, is dynamic, an ongoing process. And thus the respondent is not left, right, or center in any fixed sense, because he or she is potentially all of those things, depending upon which considerations are drawn at the time.

I am suggesting that the fixed position conception is wrong. But I do not wish to say that it is silly, or that we should have known better. The idea that we confront conflicting considerations and resolve them into a personal stance, which remains fixed, at least until important circumstances change, is not silly. It is probably an excellent account of how those professionally involved in public affairs do operate. If it is important to us to have a position and to know it, then there is real economy in operating this way. The difference for the ordinary citizen, of course, is that it rarely is important to have a personal stance and to know it. The most economical way to operate is not to think much about resolving complex considerations until pressed to do so by the nice lady in the living room, itself a low probability event.

What is critically important in this conception is that attitudes are ambivalent; each of us carries around competing considerations. Each citizen has a range of views, depending upon which consideration is consulted, not a single fixed one, and that range tends to be large. This produces expressed attitudes which will differ over time and circumstance and be sincere—sometimes even intense—on each occasion. This immediately makes sense of sundry findings of substance and method in survey work. And issues of priming and framing, which seem anomalies from the fixed position framework, are logically necessary for the Zaller conception; they explain, when many considerations are possible, which are actually drawn.

When we pose a survey question to a Zaller respondent, we get an answer which is one of several that are possible. The response doesn't tell us the respondent's fixed position, for that probably doesn't exist. But different people will have different distributions of possible responses, and the response we actually collect provides some limited information on what that distribution might be. If the considerations were drawn randomly—without priming from history or the survey instrument, for example—then we can aggregate over either responses or individuals and begin to have rich information about those distributions.

This is political psychology at its best. It is good micro theory that also has immediate macro implications. This is where Zaller speaks to Mayer and Gamson. "You can't legislate morality," we used to say, referring to the immense difficulty of changing cultural prejudices about race and gender. And yet these sets of attitudes have changed quite dramatically, as Mayer shows, and this includes change among the age cohorts whose views were thought to be fixed when we "legislated" change. This is hard to understand from a fixed position framework. From Zaller's ambivalence notion we are nearly all part racist, part sexist, *and* nearly all part believers in equality. We carry around considerations that can push us in either direction, finally "fixed" only in the grave. All of these views are genuine. The sexist who asserts belief in absolute equality is not lying, but rather holds to this equality consideration at the same time as others which produce sexism. Social change can implicitly serve to activate some considerations, and thus to change attitudes. This process of activation of considerations is much of what Gamson calls consciousness.

One test of the power of any theory is this: Can you read and understand it and then still see the phenomenon as you did a priori? Most theories pass—which is to say, fail—this test. After reading Zaller, I can't ever go back to pre-Zaller views of public opinion and survey research. I see my own work through different eyes. What I find really remarkable about this (although an historian of science probably would not) is that there is nothing new in this whole structure except the structure itself. Zaller's building blocks are well established stuff. The insights of modern cognitive psychology are far from new. The idea of competing frames is as old as rhetoric. The list of fixed position anomalies in survey research could have been fully drawn in the 1960s; indeed Zaller is consciously carrying on a dialogue with Converse and McGuire writings of the 1960s. What this book does is build theory, and not merely good theory. This is powerful stuff.

POLITICAL THEORY

Fundamental Feminism: Contesting the Core Concepts of Feminist Theory. By Judith Grant. New York: Routledge, 1993. 226p. $52.50 cloth, $16.95 paper.

In *Fundamental Feminism,* Judith Grant articulates and develops a provocative set of claims regarding Anglo-American feminism and the contemporary feminist project. Anglo-American feminist theory, she argues, is far more unified than divided. Undue attention to the varieties of hyphenated feminisms (e.g., liberal-feminism, radical-feminism, socialist-feminism) begs the important question, What makes feminist theory specifically feminist? Grant proceeds to answer this question with respect to two distinct versions of feminist theory. The first is a discernible tradition of Anglo-American feminism whose constituent core concepts include *Women, experience,* and *personal politics*. The second is a refurbished feminist theory informed by "the idea of an ideological structure of gender from which we seek liberation" (p. 189). Grant's effort on behalf of "a revitalized feminist theory" functions simultaneously as "a theory against feminist theory" (p. 14). The burden of Grant's argument lies in her claim that the second—and preferred—version of feminist theory is analytically distinguishable from its Anglo-American relative because it has dispensed altogether with the first two of the core concepts and treats the third with cautious consideration.

In three out of the five substantive chapters, Grant develops her claim that several varieties of Anglo-American feminism (socialist feminism, mothering theory, early and contemporary radical feminism, standpoint theory, et al.) share a conceptual structure anchored in three core concepts that were first introduced by early radical feminists in the late 1960s. As a result of the infamous intransigence of New Left men, the idea of a specific women's oppression theoretically analogous to modes of oppression recognized by the New Left was invented by disaffected women of the New Left. The idea that women are oppressed as women implied the recognition of women as a collectivity. This, in turn, generated a theoretical quest for the basis of connection among women, which landed feminists into the quagmire of essentialism when Woman emerged as the rhetorical figuration of women's shared oppression as women.

In order to sustain the claim that all women are similarly oppressed, within a political and intellectual environment that offered no resources for the "objective" (i.e., theoretically rigorous) depiction of this oppression, experience was invoked as that which united women. In a related and now familiar innovation, the purview of the political (notions of institutions, oppression, coercion, and rules) was expanded to include practices and habitats labeled as personal. Among the three core concepts identified by Grant—woman, experience, and personal politics—the first two in particular have generated consequences of disastrous import for feminist theory and politics. Whom does "woman" refer to? What is implied in the term experience? And what is to be included in personal politics? According to Grant, "This triage of questions remains the albatross around the neck of feminist theory. The task of answering these questions has proved endless" (p. 39).

The problem with experience as a foundational concept is that women's experiences do not always unite them. The quest for experiential unity has issued, notoriously, in predominantly white and often middle-class versions of "women's experience." In a related vein, the counterquest for "authenticity" issues in the proliferation of nonnegotiable differences among women. "Identity politics" is the inevitable result. And then there are politically conservative antifeminist women whose "experience" must be repudiated. How to account for women whose experiences do not accord with those of others? Which experiences should be prized as central and valid? Which do not qualify as such, and why?

These problems, according to Grant, inspired the quest for a feminist epistemology that could account for women's experiences and provide some guide for the production of certifiably feminist knowledge regarding women's experiences. Feminist standpoint theory, however, is enmeshed within a tautological quandary: it claims to be a theory of knowledge based on women's lives, but it employs a feminist lens to selectively describe those lives, which are then invoked as the foundational basis for feminist principles. According to Grant, feminist standpoint theory disingenuously constructs the "experiences" that it invokes as its foundation.

In the final two chapters, Grant fashions an escape from the quagmires of essentialism and epistemology with the aid of postmodern theoretical resources and contemporary feminist sensibilities. It is time to substitute "the point of view of human liberation from gender" for "the point of view of an imagined female experience" (p. 161). Grant articulates, explores, and applies a number of hypotheses regarding gender as a power structure in order to show just how Woman and experience can be displaced without compromising the notion of feminism as a specifically political practice. Yet contrary to her claim that she was able to offer feminist analyses of several films "without once talking about female values, the feminine experience, or women's experience(s)" (p. 175), Grant stumbles.

In a telling observation that may actually concede a bit more than she intended, Grant claims that the substitution of gender for women's experiences as the focus of theoretical inquiry is viable because, unlike the early radical feminists, we "do not have to prove the existence of women's oppression" (p. 160). What Grant does not spell out is that we owe some debt of gratitude to the core concepts for this contemporary state of affairs. In effect, the move from "female experience" to "gender" may not be quite as cleanly distinguishable as Grant suggests. This unsettling possibility is displayed in Grant's own discussion of the two famous shooting scenes in the film *Thelma and Louise*: "The shooting of the tires is *every woman's fantasy* of how to respond to sexual harassment, and, if I may say so, the shooting of the would-be rapist is probably *our collective fantasy,* too" (p. 168, emphasis mine). Grant herself has succumbed to the seduction of the core concepts (Woman and experience) in the final chapter of her book, entitled "Solutions," where she proposes to show her readers how to do feminist theory without recourse to the core concepts. This invites a reconsideration of Grant's claim that a clean break with the core concepts is both feasible and necessary and that the move to gender as the focus of feminist analysis constitutes just such a break.

A core achievement of *Fundamental Feminism* is that it alerts us to the important notion that experience cannot assume foundational status, because it is "always-already" theoretically preselected or constituted. But the question of whether experience might occupy alternative space within feminist theorizing still lingers. Doesn't the salience of gender as a category of analysis still rely on touchstone notions of experience? In a related vein, the charge of tautology, leveled relentlessly by Grant against feminist standpoint theory, will not persuade those who subscribe to the view that tautology is an inescapable feature of all theorizing. For these and other reasons, I think that *Fundamental Feminism* succeeds as a thoughtful and timely theoretical explication of a salutary shift in focus already under way within feminist theory and politics, and rather more organically related to its early radical feminist roots, rather than of a pure substitute formation.

University of Washington CHRISTINE DI STEFANO

The General Will: Rousseau, Marx, Communism. By Andrew Levine. New York: Cambridge University Press, 1993. 222p. $49.95.

The collapse of actually existing socialism in Eastern Europe and the Soviet Union has exacerbated a longstanding "crisis of Marxism," but it has simultaneously reopened new paths of speculation about alternative models of socialism. Andrew Levine is acutely aware of both these tendencies, but he is particularly concerned that the demise of the Communist party and the decline of Marxist theory on the Left has resulted in "a motley of good causes, free of any coherent social vision" (p. 219). Thus, Levine's overriding concern is to reestablish the normative basis for a social vision of communism. Levine seeks to lay this foundation by arguing "that the idea [of communism] is not nearly so utopian or indefensible as many, including many on the left, nowadays assume" (p. 22).

In developing his thesis, Levine suggests that Marx's idea of communism can be further clarified as a normative ideal by drawing on Rousseau's concept of the general will. The core of his argument is that "communism in Marx's sense is Rousseauean sovereignty attained or rather overcome, since what Rousseau's social contract establishes is not, in the end, a state at all but, as Kant would have it, a 'republic of ends' " (p. 5). The most engaging chapter is Levine's exploration of the last state, where he discusses the possibilities and conditions for a transition to communism, that is, a stateless society. Since the last state is required to undermine the conditions of its own existence by promoting an end to the state, Levine argues that "liberal tolerance is a likely feature of any well-functioning proletarian class dictatorship" (p. 132).

Levine conceptualizes the normative possibility of a transition to communism by distinguishing between *methodological* atomic individualism and *substantive* atomic individualism. The former is the idea that social and political philosophy starts from a conception of the individual and his or her interests, while substantive atomic individualism is the view that individuals are radically independent centers of volition (p. 26). According to Levine, the latter conception of individualism must be dislodged from political theory in order to conceptualize the normative possibility of communism. The former conception establishes a bridge between the Rousseauean general will and individual action. Thus, he suggests that if one grants the premise that economic scarcity is "substantially overcome" under communism and that self-interest is "rendered benign" by his conceptual distinction, people will "set about to become the free (autonomous) beings that, according to Rousseau's vision, they always essentially were" (p. 198). More to the point, Levine, argues that the "overwhelmingly preeminent concern" of individuals living in a communist society will be "the enhancement of autonomy" (p. 198).

Unfortunately, Levine's central thesis is developed in fits and starts, because he is also intent on demonstrating simultaneously that liberalism is "more compatible with political positions traditionally held by radical egalitarians and Marxists than was previously believed" (p. 152). Although a worthwhile endeavor, defending this second claim results in endless diversions into Hobbes, J. S. Mill, utilitarianism, rights-talk liberalism, rational choice and social choice theory, and other varieties of methodological individualism. Moreover, Levine's frequent diversions into rational choice theory do less to clarify his original thesis than a straightforward analysis of texts by Marx, Rousseau, and Kant. Instead, the frequent attempt to translate authors into a language that was alien to them merely interrupts and breaks the development of a potentially engaging argument. The author and readers would have been better served by a much shorter and tightly argued essay that remained focused on its original thesis. In many ways, Levine's book replicates the motley state of contemporary radical theory without achieving the anticipated synthesis and without introducing any substantial theoretical innovations that take Marxism beyond its disoriented condition.

University of Massachusetts, Dartmouth CLYDE W. BARROW

Micro-Politics: Agency in a Postfeminist Era. By Patricia S. Mann. Minneapolis: University of Minnesota Press, 1994. 253p. $44.95 cloth, $17.95 paper.

"A postmodern, postfeminist era has begun," declares Patricia Mann in her ambitious attempt to articulate a feminist theory of social and political agency for our times (p. 25). In Mann's account, the social enfranchisement of women, the demise of the patriarchal family, and the decline of normative masculinity all call for new ways of theorizing the subject and its relationship to the larger community. In the context of the radical changes that have taken place in gender relations, Mann argues, liberalism no longer provides the conceptual tools for making sense of women's or men's public and private lives. The very category of the free and autonomous individual, which Mann takes to be the hallmark of liberalism, is not simply under siege but anachronistic: it assumes gendered notions of social identity and agency that no longer obtain in our so-called postmodern era.

Taking feminist challenges to patriarchal conceptions of sexual difference as "the most significant social phenomena of our time" (p. 2), *Micro-politics* points to the inadequacies of liberalism, arguing that its assumptions about masculinity and femininity do not resonate with the material reality of gender relations, a reality in which the majority of women work outside the home and men are increasingly called upon to do their share within it. Mann is surely right to emphasize the challenge that changing gender relations pose for social and political theories that distribute different forms of agency along the familiar axis of a passive femininity and an active masculinity. To the extent that liberalism conceives of women as agents at all, she argues, it defines their agency in terms of self-interest. We need a more flexible and interactive theory of agency, says Mann, one that takes account of the "relations between two or more individuals" (p. 14).

In a series of chapters examining a wide range of issues in feminism (including abortion, pornography, and sexual harassment), Mann analyzes the place of gender in our understanding of agency and in the deeply conflicted nature of postmodern subjectivity. In chapter 1, for example, Mann shows that although the "social enfranchisement of women" has given them an increased sense of autonomy, it has also placed enormous strain on women as they struggle to balance the competing demands of their private and public lives. Likewise, she argues, the movement of women into public life has called "male family identity" (p. 55) into question, but it has not yet resulted in a reconfiguration of fatherhood and masculinity.

As Mann moves to map these historical changes and conflicts for her readers, she initially analogizes the traditional position of woman to that of the slave: both are "denied the possibility of action in the world in his or her own name," both are expected "to act wholly for others" as opposed "to acting for themselves" (p. 48). This analogy, she argues, is problematic, but only insofar as it fails to acknowledge the fact that woman, in supposed contrast to the slave, was assumed to be acting for herself when she acted for others. Although early feminism criticized this ideal of a naturally self-sacrificing femininity, it failed to consider the possibility that a woman could act both on behalf of others *and* in her own name. A feminist theory of agency, Mann maintains, must provide a language for comprehending social desires and practices that appear unintelligible and mutually exclusive within a liberal feminist framework.

Mann's account of womanhood as slavery will provoke objections from readers who see in the analogy the erasure both of the woman who was quite literally a slave and of the historical specificity of racialized subjects, what it meant to be constituted as a slave in different cultural contexts. Her account of liberalism will provoke objections from readers for whom this tradition includes the works of critics of exclusively self-regarding behavior such as John Stuart Mill. By neglecting those strands of liberal theory that recognize other-regarding behavior as compatible with what Mann calls "acting in one's own name," *Micro-politics* reduces liberalism to a familiar caricature.

There are at least three other, related problems in Mann's text: (1) a tendency toward hyperbole ("Within a period of no more than twenty-five years, social norms about the economic responsibility of women within marriage have been turned inside out," p. 143); (2) an exaggerated conception of what the transformation in gender relations means for other social relations, such as race and class; and (3) a vague, if not ill-conceived, notion of postmodernism as the heroic successor to liberalism. I shall consider each of these problems in turn.

Comparing the "unmooring" of women from the patriarchal family with the "unmooring" of serfs from the feudal estate (p. 128), *Micro-politics* establishes an analogical relationship between gender and class that is as questionable as the one it posits between women and slaves. Although Mann rejects the Marxist analogy between the patriarchal relations of reproduction and the capitalist relations of production, she persists in arguing that the transformation in kinship relations is akin to the revolution in class relations that marked the birth of capitalism (as it was narrated in *The Communist Manifesto*). But if gender relations are not, in fact, like class relations (as Mann also maintains), then why press the historical analogy between the shift from feudalism to capitalism and the shift from the patriarchal to the (supposedly) postpatriarchal family? As a rhetorical device, this comparison highlights the extent of the changes wrought by the social enfranchisement of women—but at a cost. The rhetoric produces an unrealistic and exaggerated image of a triumphant feminism. Statements like "Women have made it to center stage in the historical drama" (p. 33) evince a monumental conception of historical change that is not only questionable but also at odds with Mann's alleged "postmodern," "micro-political" theory of agency.

Indeed the title of Mann's book, *Micro-politics*, belies the grand narratives sketched therein to map changing gender relations and their implications for "agency in a postfeminist era." Mann's effort to "develop a theoretical framework intended to provide insights into our complicated nexus of social relationships" is both useful and plausible (p. 1). But the grandiosity of her claim that "gendered social transformations . . . provide the basic foundation for all other social transformations today" (ibid) does not square with what goes under the sign of a feminist, postmodern approach to social change. In the writings of Denise Riley, Judith Butler, Jane Flax, and Joan Scott, for example, shifts in the relations of race and class are theorized as anything but derivative of the supposedly more foundational shifts in the relations of gender. *Micro-politics* aspires to provide "a micro-political theory of agency for multicultural sites of social transformation" (ibid.). But Mann shares many of the same problems that she denounces in liberalism and Marxism: she treats the category of women as if it were (more or less) unitary and ascribes to gender the world-historical importance that she accuses liberals of ascribing to the individual and Marxists to class. This view of gender as the foundation of all other political transformations places significant constraints on Mann's account of late twentieth-century social relations.

Some of the problems that Mann's theorization of gender raises for her self-identified postmodern approach to agency can be seen in her chapter on pornography. Consider, for example, Mann's account of "the quite radical asymmetry in biologically based male and female forms of sexual agency." Arguing that "historically, the male ability to rape and impregnate a woman provided an important ground for patriarchal sexual and social power relationships," Mann comes perilously close to naturalizing the oppression of women, the institutions of marriage and the family, and the exclusion of women from public life. She goes so far as to argue that the "immediate asymmetries indicated by male spermatic agency" (i.e., by "a man's physical ability to unilaterally act upon a woman in a socially significant way") actually "provided a material and social basis for more general forms of male domination over women" (p. 68). This male ability to rape a woman, grounded in nature or brute physical strength, is then said to anchor "the social discourses that represent men as active and women as passive" (p. 68).

The act of rape is thus cast by Mann not as the product of certain culturally and historically contingent configurations of female and male bodies, in which the former is construed as violable, the latter as violating; nor is it cast as a ritualized practice of terror that characterized the institution of American slavery, in which there was an important difference between the position of a (white) woman and that of a slave. The act of rape is cast, rather, as the biological origin or anchor of the social and political meanings of an absolute sexual difference.

Mann is not the first feminist to argue that patriarchal violence has a basis in the biological difference of the sexes. A writer like Susan Brownmiller might find intellectual kinship with the project of *Micro-politics*, but it is doubtful that the "postmodern" theorists with whom Mann would mark an affinity will be so inclined. One might conclude that the marketing of *Micro-politics* as a work of postmodern, postfeminist theory amounts to false advertising. Rather than specify what postmodernism is, however, one might say about its definition in Mann's text what Julia Kristeva once said about definitions of woman: "That's not it" and "That's still not it."

Rutgers University LINDA ZERILLI

Technology in the Western Political Tradition. Edited by Arthur M. Melzer, Jerry Weinberger, and M. Richard Zinman. Ithaca: Cornell University Press, 1993. 333p. $38.50 cloth, $15.95 paper.

Technology (i.e., the intellectual-practical revolution that defines modernity as a global project of subduing and instrumentalizing nature) is widely if not universally conceived as marking a decisive watershed in the history of Western consciousness. The editors, all three of whom are political theorists at Michigan State University, have put together an engaging collection of essays devoted to this theme, drawing upon a conference and lecture series they organized during 1989–90 at the same institution. The volume is attractively wide-ranging, for the authors practice radically different modes of theory. As is to be expected, the ways in which they address the issue of technology reflect their very different theoretical concerns, with the consequence that some of the essays stray a bit from their assigned topic. All of them, however, are instructive in their various ways.

Leon Kass, in a fine introductory essay, cautions the reader that while theorizing may help to make us more reflective about how our present predicaments arose, it would be asking too much to expect it to show us the way out of these predicaments. In fact, the very designation of technology as a "problem" (hence implying the promise of a "solution") is itself a sign of technological thinking.

Several of the more interesting contributions are revisionist endeavors. Where do we look for intellectual alternatives to the modern technological spirit? Standard answers would be: pagan antiquity, medieval Christianity, the romantic critique of the Enlightenment. But three of the authors in this collection try to show, in each of these cases, unsuspected complexities in the relationship between these supposedly pretechnological or antitechnological intellectual worlds and our modern embrace of technology's powers. Paul A. Cantor, in an excellent essay on romanticism and technology, follows the lead of Mary Shelley's *Frankenstein* in tracing "affinities between Romanticism and the technological impulse" (p. 121). There is a rather ill-tempered essay by Stanley L. Jaki on modern technology's debt to the medieval rejection of a pantheistic cosmology. (The ill temper derives from Jaki's conviction that medieval Christianity has not received the credit due to it for its liberating influence within the history of technology.) Finally, in the same revisionist spirit, we get a quite penetrating essay by Stanley Rosen that explores how "Platonic pessimism" anticipates the inspiration behind modern *poiesis*. Rosen suggests that Plato, in his thinking about the relation between theory and production, is closer to the "practico-productive theory" of Baconian-Cartesian modernity than is commonly thought. According to Rosen, Plato, with his "poetical discontent with the place of human beings in the cosmos," offers "a lightly masked representation, couched in the tones of Attic urbanity, of the incipient spirit of modernity" (p. 83). James H. Nichols, Jr., on the other hand, presents a more conventional view: relative to the *sophrosyne* championed by Sophocles, Thucydides, and Aristotle, modern technology appears as pure hubris.

Wilson Carey McWilliams offers us one of his characteristically eloquent responses to the American political tradition. His essay depicts America as a "technological republic" whose founders sought to depend upon the mechanisms of interest ("political hydraulics"), rather than upon the attachments that form the soil in which civic virtue is rooted—thereby depriving the American republic of vital sources of moral direction and self-restraint. Just as McWilliams argues that reliance on "political technology" has attenuated moral reflection within the American republican tradition, so G. A. Cohen argues that Marx's reliance on a "technological fix" (the assumption of utopian levels of productivity and work satisfaction) diminishes the moral resources available to the Marxist tradition. According to Cohen, the Marxian presumption of technologically guaranteed material abundance allows those in the Marxist tradition to sidestep issues of distributive justice and mutual obligation that have been taken more seriously by leftist liberals and social democrats.

Other contemporary sources that might be thought to offer possibilities of intellectual resistance to the technological regime include the Frankfurt School, Heidegger, and postmodernism, duly explored in essays by Sheldon Wolin, Reiner Schürmann, and Gianni Vattimo, respectively. The last of these three essays concludes with the peculiar suggestion that the electronic technologies granted to us in the age of information can release us from the grip of modernity, at long last ending the curse of foundationalism and heralding "the new dawn of postmodernity" (p. 224, cf. 250–51).

The volume concludes with three thoughtful essays (by William Galston, Jerry Weinberger, and Arthur Melzer) that address more directly the political relationship between technology and contemporary liberal democracy. All three are basically defenses of liberal democracy in the face of theoretical challenges that have arisen in recent decades, including challenges articulated in some of the preceding chapters of the volume. The closing essay by Melzer urges that we avoid letting ourselves get too carried away by anxiety over technology's grip upon us, since to do so would be to lose sight of the possibilities of autonomous political agency and practical choice that modern liberalism makes available. Melzer, in agreement with several of the other contributors, seems to be of the view that we need to go back and read Plato in order to keep things in perspective.

The overall message of the volume is that in order to reflect seriously on the problem of technology, one is obliged to initiate a comprehensive engagement with the Western philosophical tradition in its entirety. This is certainly a reasonable undertaking. It is, of course, an entirely different question whether any of this can actually redeem what Wolin neatly dubs "the kingdom of means."

University of Toronto RONALD BEINER

Rules, Games, and Common-Pool Resources. By Elinor Ostrom, Roy Gardner, and James Walker. Ann Arbor: University of Michigan Press, 1994. 392p. $55.00 cloth, $18.95 paper.

This is an unusual book. Ostrom, Gardner, Walker, and their colleagues analyze commons problems via both formal and informal theory and study them empirically via experiments and field research. Doing all this is difficult. It requires bringing together scholars from diverse backgrounds and disciplines, focusing their collective efforts on a common topic, and integrating their results. It is therefore not surprising that though we often praise the idea of bringing multiple methods to bear on a problem, we don't do it very often. Ostrom et al. not only attempt it; they bring it off with considerable success. This is no accident. It was the result of the careful building of a research team and a huge data base of field cases—probably the biggest data base on commons problems in the United States. (It is to be expected, of course, that such an ambitious interdisciplinary effort will not be completely successful. In particular, the field work is less tightly tied to theory than are the experiments. More on this later.)

The book is conceptually clear: its central phenomenon—common pool resources (CPRs)—is clearly defined as those goods for which excludability is difficult and consumption is rival (as contrasted with public goods, for which consumption is nonrival), and its central problem—the performance of decision makers in CPRs—is well-identified. The book's analysis of CPRs rests on two theoretical pillars: one (noncooperative game theory) is formal; the other (institutional analysis and development [IAD]), informal. Game theory needs no elaboration here; IAD does. IAD is somewhere in between a fully developed theory and a typology. It is less than a developed theory in that it does not offer explicit hypotheses. Hard core social scientists, those believing that even frameworks must generate hypotheses if they are to have any scientific value, might therefore dismiss the IAD approach as constituting a mere list of important variables. And sometimes it does degenerate into a filing system for variables: figure 2.2, for example, leaves nothing out; hence it has no predictive content.

But such a dismissal of the IAD framework would, I believe, be premature for two reasons. First, it is valuable even without generating testable hypotheses. The IAD's categories about rules that govern CPRs—rules defining position, access to the commons, and so forth—probably served an enormous cognitive function in organizing the voluminous information gathered in the field. Anyone who has confronted the task of organizing and comparing qualitative data gathered by different researchers, in vastly different field sites, must appreciate clearly defined categories. There is, after all, something to be said for good filing systems! Second, IAD is more than a typology in that its categories of rules are not exhaustive; hence they are implicitly based on hypotheses about which rules are sufficiently important to warrant data-gathering. (Indeed, unlike an exhaustive typology, the authors' set of rules is open to empirical challenge: why these rules and not others?)

Admittedly, it would be a good thing to derive more testable predictions from the IAD framework. This, however, would not be easy, especially as it is argued that the effect of the rules depends upon their complete *configuration* rather than being simply additive. If we take this configural hypothesis seriously, there are a huge number of possibilities to examine (e.g., if there are 10 types of rules, and each can take on just two values, there are 1024 different configurations). Yet perhaps the situation is not as bad as all that; there are suggestions in the empirical chapters that thinking about rules configurally may introduce unnecessary complexity. For example, in the chapter on California water, the text accompanying table 13.4 implies that the probability of a cooperative outcome (or the amount of cooperation) is increasing as one goes from NO to YES in the table's rule categories. This is a much more testable, non-configural hypothesis.

The theory chapters are clearly written and accessible to first year graduate students and advanced undergraduates. (The authors' propensity to laying out formal definitions, however, may have been carried a bit too far in chapter two: the discussion of the elements of "action situations" is somewhat pedantic. But given that this chapter develops their important IAD framework, perhaps it was better to err on the side of too much detail.) Students with no background in game theory will have to slow down considerably when reading chapters three and four, but that is because the argument is dense; the math, which is kept to basic algebra, is not difficult. Indeed, it is precisely students without formal training who should be encouraged to work their way carefully through these chapters on game theory: doing so will clarify their thinking about commons problems significantly. In particular, chapter three contains an enlightening section on when CPRs are *not* prisoner's dilemmas (PDs), possibly contrary to students' beliefs that all such problems are N-person PDs, and chapter four provides a very useful analysis of how particular rules (e.g., first in time, first in use) affect the equilibria of games. If therefore helps to integrate IAD with noncooperative game theory by showing how institutionally relevant rules matter theoretically.

The experiments yielded interesting results; two are worth noting. First, although the theoretical literature on repeated PDs has focused heavily on trigger strategies, subjects rarely use them. Indeed, subjects resisted using collective punishment strategies of *any* kind. Instead, they almost invariably tried to impose *selective* punishments—even when the identity of defectors was unknown! (In this circumstance the subjects verbally abused the unknown cheater. Clearly this is an effective punishment only if the cheater has internalized the relevant norms.) Second, although in the aggregate behavior was sometimes close to the Nash prediction, individual behavior was rarely Nash. These results probably constitute a good example of the effects of averaging idiosyncratic error. Errors there certainly were, for calculating the optimum strategy was difficult in most of the experiments, and the authors do not shy away from concluding that bounded rationality mattered.

Compared to the experimental findings, several of the conclusions from the field studies lacked punch. It is hardly surprising that successful handling of CPRs is more likely when there is monitoring and enforcement of rules regarding, for example, access to the commons (p. 315–316). A more subtle problem attends findings such as the endogeneous creation of rules and institutions, which the authors emphasize repeatedly. From the perspective of conventional political science, this is not surprising: after all, we spend a great deal of time studying how laws (rules) and political institutions are created and maintained. It may surprise experimentalists, who are accustomed to running tightly controlled experiments in which subjects, precluded from changing the rules of the game, are forced to play the PD in its starkest form.

Perhaps the experimental findings were more interesting because the lab studies were aimed at a clear target—the relatively falsifiable hypotheses of games theory—whereas the field research was mostly guided by the less testable IAD framework. Indeed, the tie to game theory in the field study chapters was often slender. The formal analysis that was used was restricted to oneshot games, though the phenomena clearly involved repeated interactions.

Probably because the field data were less powerful than the experimental results, the authors could more easily maintain strong beliefs in hypotheses about macro phenomena—how real institutions manage CPRs—than about micro phenomena—how individuals behave in the lab. One of their most important propositions, maintained throughout the field studies, is essentially that local governance is good, central governance is bad. They do adduce evidence to support the claim, and their theoretical argument for local governance—institutions close to the problem are more likely to have information required for effective control of CPRs and to adapt to changes in those problems—is plausible enough.

But the assertion is too sweeping. There is virtually no theoretical analysis of the disadvantages of local governance or the benefits of central governance; it is as if the former can do little wrong and the latter little right. Nor is there consideration of how central institutions might work in concert with local ones—although this possibility has been explored in the literature on institutions and the repeated PD (e.g., Milgrom, North, and Weingast's work on the institution of the "law merchant" in medieval Europe). And empirically, they provide data (largely ignored) that contradicts the sweep of their own claims. For example, in several of the Indian villages studied in chapter 12, the locally dominant Brahmins run the commons for their benefit and exploit the lower castes. And this is not an arcane issue in political science: it is not unusual for local elites to exploit people of lower status, and the latter sometimes find an ally in the central government. A second example: in the California water case it is casually noted that "California pumpers have access . . . to very good technical information provided by overlapping state and national agencies" (p. 308). Though most of the book pays careful attention to informational problems in CPRs, the significance of centralized informational assistance is overlooked in the California study.

In general, however, the authors are admirably receptive to disconfirming evidence. In particular, they freely acknowledge that several game theoretic predictions are confounded by experimental results, and they do not introduce ad hoc modifications in order to "save the theory." Thus the book is a fine counterexample to the claims of Green and Shapiro in their recent critique of rational choice theory.

We will and should see more collaborative efforts of this kind. There will be both a push and a pull for this trend. The pull is that the triangulation afforded by combining field research and experimentation, with both informed by theory, will be too tempting to forego. The push is that it will become increasingly difficult to become adept at all these methods. The solution is more successful collaboration of the kind produced by Ostrom, Gardner, Walker, and their colleagues.

Stanford University JONATHAN BENDOR

Intuition and Construction: The Foundation of Normative Theory. By T. K. Seung. New Haven: Yale University Press, 1993. 227p. $27.50.

In his dialogue *Theaetetus,* Plato argues for transcendental standards of knowledge—Ideas, or Forms—indirectly. Without invoking such standards, Socrates and his interlocutors run through a number of possible accounts of knowledge, all of which are seen to fail. Thus, the existence of Forms emerges from the unacceptable consequences of not having them. In *Intuition and Construction,* T. K. Seung argues along similar lines in order to demonstrate the existence of transcendental moral standards, which he also identifies as Platonic Forms.

Through seven of the book's nine chapters, Seung examines recent moral theories and the view of Kant. He is most interested in different forms of "constructivism," by which he means "the thesis that normative propositions and standards are constructed by human beings" (p. ix). Different versions of constructivism are attributed to a range of thinkers, including R. M. Hare, Alan Gewirth, Jürgen Habermas, and David Gauthier. Basically, Seung argues that these theorists violate their own constraints, illicitly introducing substantive propositions into their purportedly purely formal theories, in the former three cases, while Gauthier is unable to circumvent the "obvious extensional discrepancy" between his own view and traditional morality (p. 108). The "ideal" constructivism of John Rawls is carefully examined, with different strands of Rawls's theory sorted out. Though somewhat sympathetic to Rawls, Seung criticizes him for taking his starting points from existing societies, thereby falling into nonnormative moral conventionalism. Then in attempting to avoid this situation, with his "Kantian constructivism," Rawls does not go far enough in recognizing transcendental norms.

After discussing additional thinkers, including Alisdair MacIntyre and Aristotle, Seung devotes a detailed and learned chapter to Kant. He contends that Kant abandoned the formal strictures of his *Groundwork of the Metaphysics of Morals* in his later works, notably the *Critique of Judgment*, in which he invokes Platonic Ideas. While Seung's recounting of Kant will undoubtedly provoke controversy, his critical examinations of all thinkers are trenchant and highly interesting. His summaries are clear and compact and will benefit virtually all readers; the book can be recommended on this account alone. The discussion of constructivism is particularly rewarding, especially for students of Rawls, who will learn much about his moral method and how it relates to the views of other recent thinkers.

However, Seung's lengthy examination of these thinkers must be regarded as a prelude to his most original and provocative argument, that a revival of Platonic Forms can overcome the difficulties encountered by other scholars. Success here would obviously be a major contribution to ongoing debates. But the bulk of this side of Seung's project turns out to be more exposition, in this case of the metaphysics of Plato, who is said to have presented a defensible theory of Forms in his late works. Seung argues for a development of Plato's thought between the *Republic* and later works, notably the *Statesman* and *Laws*. He claims that Plato replaced the "skyscraper" account of Forms in the *Republic* with a less objectionable "bedrock" version. In the skyscraper model, the moral Forms provide fully developed moral standards, applicable to all conditions. In the bedrock version, Forms are indeterminate, providing only a normative foundation that must be filled in through application to particular circumstances, or construction. Thus, Seung's view is that Platonic Forms, known by rational intuition, must be appealed to in order to provide the secure base on which successful moral construction can proceed—hence the title of the work, *Intuition and Construction*.

In addition to the authority of Plato, Seung argues for his view by appeal to the consequences of forgoing Platonic moral standards, which are unacceptable: "Without transcendental norms, we would be imprisoned in normative positivism and cultural relativism" (p. 215).

Not surprisingly, I do not believe Seung's project ultimately succeeds. First, Seung's heavy reliance on an admittedly "highly unconventional reading" of Plato (p. xiv) is disconcerting, especially because he does not defend it in detail. For example, there is strong evidence for the reappearance, in book 12 of the *Laws*, of Forms akin to those described in the *Republic*. The implications of the language in this portion of the *Laws* are forcefully argued by Harold Cherniss (in the 1953 issue of *Gnomon*), among other scholars. Another problem is that Plato's Forms are components of an entire worldview, bound up with the immortality of the soul, the doctrine of recollection, and other familiar Platonic views. How the Forms can be extracted from this theoretical edifice is a problem Seung does not address. Accordingly, even if the authority of Plato did have immediate cachet in contemporary moral philosophy, the fact that Seung ignores both difficulties with, and evidence against, his interpretation of Plato obviously weakens his case.

Second and more important. Seung's main substantive argument is also undeveloped. The consequences of not accepting his transcendental standards receive short shrift. It is not clear that a moral method based on coherence (or reflective equilibrium, popularized by Rawls) cannot avoid the chaos Seung foresees. By working from our most secure convictions and the moral principles in which we have most faith, shaping this material into a coherent structure, one could develop a perspective from which to criticize objectionable aspects of our own culture, as well as other cultures. This possibility, widely supported in the philosophical community but ignored by Seung, poses nonmetaphysical counterpart to his ascent to Platonic heaven.

Third and last is a question of proportion. In Seung's critical chapters, thinker after thinker is skewered for resorting to unjustified assumptions. Then Seung argues from this accumulation of unsuccessful attempts to the need for his own position. But such an argument from elimination is unconvincing. In particular, it remains to be shown how the failure of these other thinkers justifies Seung's introduction of far more radical and controversial assumptions into his own position. Needless to say, the Platonic essences he posits must be carefully explained and defended in detail. Seung's failure to develop the positive side of his case properly is a significant shortcoming of a book that otherwise has much to recommend it.

University of Virginia GEORGE KLOSKO

AMERICAN POLITICS

Values and Public Policy. Edited by Henry J. Aaron, Thomas E. Mann, and Timothy Taylor. Washington: Brookings, 1994. 216p. $29.95 cloth, $11.95 paper.

Public Policy for Democracy. Edited by Helen Ingram and Steven Rathgeb Smith. Washington: Brookings, 1993. 274p. $34.95 cloth, $14.95 paper.

The shared premise of these two books is that despite the expenditure of hundreds of millions of research dollars and the best efforts of many, policy-oriented social science has fared badly. Social scientists have produced few policy-relevant ideas, explanations, or theories that are at once intellectually compelling, practically useful, and demonstrably effective. And rather than offering guidance about how public policies can be fashioned so as to enhance civic education, stimulate enlightened debates, or broaden opportunities for meaningful participation in the democratic process, social scientists have tended to leave average citizens, the private sector, public-private partnerships, and democracy itself out of the equation.

Both books are publications of the Brookings Institution, long the country's leading public policy think-tank. Thus, they have the ring of self-indictments. The recurring charge is not purposeful intellectual malfeasance but persistent intellectual misdirection. In the end, however, these books offer not only searching criticisms but a set of generally persuasive prescriptions for changes in how policy-oriented social scientists do what they do.

In sum, the volume edited by Henry J. Aaron, Thomas E. Mann, and Timothy Taylor beckons social scientists to research how people's values are formed and how values can mediate the effects of public policies. Likewise, the volume edited by Helen Ingram and Steven Rathgeb Smith harkens social scientists to pay greater attention to how average citizens' democratic impulses can be stimulated or stymied by different types of policy decisions and institutional designs.

According to the three editors of *Values and Public Policy* (Aaron and Taylor being economists and Mann a well-known political scientist), social scientists have confined themselves to evaluating public policies and institutions by examining tangible (most often pecuniary) incentives and behavioral responses. First, the good news: by taking values as givens, by proceeding as if human behavior were narrowly self-interested maximizing behavior subject to more or less readily quantifiable institutional and other constraints, social scientists have been able to offer parsimonious explanations of policy-relevant phenomena and to build elegant mathematical behavioral models of the same. As a bonus of this approach, most analysts have avoided getting caught up in emotionally charged debates in which rank racial, ethnic, or cultural stereotypes are substituted for rational explanations of social and economic problems.

Now, however, the bad news: this approach is "wrong, (or) at least oversimplified" (p. 3) For good analytical reasons, social scientists tend to be mum on values. But many policy debates dwell "precisely on alleged differences in values and norms that people believe lead to social and economic problems" (p. 2). While many "appeals to values in public policy debates are facile, simplistic, judgmental, and ill considered," average citizens have a point (p. 3). Values are not exogenous; values can change; responses to policies depend on values. And together with other influences (families, friends, schools, churches), public policies can "change values directly or indirectly and thereby change the responses to public policies themselves" (ibid.).

Crime policy furnishes an excellent example. In the 1980s and early 1990s, many states made the penalties for street crime more swift, certain, or severe. As the penalties increased, criminal victimization rates declined slightly. But crime rates in urban America hardly budged, while violent crimes including homicides among inner-city juveniles reached epidemic proportions.

In chapter 3 of *Values and Public Policy*, James Q. Wilson argues that variance in crime patterns cannot be understood without reference to values. In the 1970s, Wilson was preeminent among those who argued that ceteris paribus, if enough states made the penalties for street crime tougher, then America would have less street crime. But even in the 1970s, Wilson's argument contained a crucial caveat. Any such incentives-based attack on crime, he predicted, would result in only a small reduction in the rate of serious crime. If all would-be street criminals were forward-looking rational actors, that would be one thing. But the reality is that many criminals are untroubled by the stigma of arrest, the pains of imprisonment, or the pangs of conscience. Many harbor values that make them radically present-oriented and self-regarding. In particular, many inner-city young males are caught up in a drug-and-crime culture in which being arrested is a rite of passage, going to prison is a badge of honor, showing no mercy to victims is a measure of maturity, and few expect to live past the age of 30.

Wilson blames the inner-city crime crisis on the lack of two-parent families that impart the values of a civilized, noncriminal way of life. "Our object," he avers, "ought to be to increase the number of urban young men who marry and remain married" (p. 74).

But giving values a central place in policy studies does not preordain a conservative conclusion. In chapter 4 of *Values and Public Policy*, David Popenoe analyzes family policy. Although he does not tackle the issue directly, his analysis challenges conservatives' favorite incentives-based argument on welfare. That argument runs essentially as follows. Welfare spending destroyed the black urban family; the destruction of the family has given rise to increasing rates of out-of-wedlock birth, which in turn have fueled youth crime and other social problems. If welfare is curtailed or ended, illegitimacy rates will drop, both because poor single mothers won't be able to afford additional children and because families, friends, and the community-at-large will once again stigmatize young people who behave irresponsibly.

From Popenoe's analysis, however, it seems clear that neither the general condition of the American family nor the virtual demise of the black inner-city two-parent family can be explained in such simple terms. Problems with the "family condition of America," he argues persuasively, are "at heart cultural, not political or economic" (p. 102). For example, in inner-city neighborhoods where teenage pregnancy has become the norm, many young women view having children with or without the benefit of marriage as a legitimate way of acquiring greater self-esteem and social status. And many a young man in these neighborhoods grows up thinking that any children he fathers are not rightly his personal and financial responsibility. The fact is that as welfare payments have declined in real terms, illegitimacy rates among welfare recipients have increased. But in light of the sort of values-based analysis of the family outlined by Popenoe, the notion that policies that manipulate monetary incentives can magically restore the two-parent family that loves, nurtures, and socializes its children is not only statistically suspect but downright fanciful. Thus, a values-centered policy analysis promises to be an antidote to facile, incentives-based social engineering on the Right, as well as on the Left.

Nor should it be supposed that a focus on values means that social scientists must forsake rigorous economic models for diffuse sociological musings. In chapter 6, economists George Akerlof and Janet Yellen offer a model of crime and punishment in which the willingness of community members to report crime and inform on gang members is central to the success of law enforcement. The Akerlof–Yellen model (utility equations and all) represents a welcome refinement to the economic theory of crime advanced in a classic 1968 article by economic theorist Gary Becker. More than that, it furnishes a nontrivial theoretical justification for the view that "manipulation of social values is as important in the control of crime as harsh punishments and high public expenditures for police" (p. 174).

The rest of this volume includes Daniel Yankelovich's interesting statement on how changes in the economy are reshaping American values (chapter 2), Nathan Glazer's provocative ideas about the future of multiculturalism in America (chapter 5), and Jane Mansbridge's exploration of public-spiritedness in the United States (chapter 6). Despite its many strengths, there are at least three reasons why *Values and Public Policy* should be marked Handle with Care.

First, the book underplays the importance of religious values in American life. Most Americans believe in God, pray on a daily basis, acknowledge absolute standards of right and wrong, and believe that individuals are strictly accountable for their actions. As the editors note, average citizens are inclined to "blame bad people for social problems and to minimize the role of circumstances, institutions, or laws" (p. 3). Most Americans, unlike most social scientists, have, as it were, been influenced by their religious beliefs to resolve the problem of free will versus determinism wholly in favor of the former. In contrast, policy scientists want to have their causal cake (behavior is a function of antecedent causes) and eat it too (individuals are personally responsible for their deeds). But as Isaiah Berlin and others have demonstrated, it is simply not rational both to believe that choices are caused, whether by structural factors directly or mediated through "values," and to treat individuals as deserving of praise or blame, rewards or punishments, for choosing to behave or refrain as they do. A values-centered policy studies has every reason to take religion seriously, resisting any temptation to relegate core religious concepts, beliefs, and values to a limbo of lesser intellectual, moral, and behavioral significance.

Second, the volume never quite confronts the truth that policy analysts have a divided mission, perhaps an impossible one. Producing statements about public policy problems that are at once true empirically, interesting intellectually, and useful practically is difficult because these three goals are normally in conflict.

Third, the editors should have been even more forthright in acknowledging the long list of failures wrought by of the lack of emphasis on values within the social sciences. A good place to start would be the failure to explain such "irrational" behaviors as why some people give to charities when they derive no direct benefit from doing so; why public officials ever behave in a public-spirited manner; why fire fighters and soldiers voluntarily give their lives to save others; why some poorly paid government workers refrain from shirking, subverting, or stealing on the job even when they know they could get away with it; and so on.

The Mansbridge chapter carries this weight for the volume, but one wishes that editor Aaron's superb essay "Public Policy, Values, and Consciousness" (*Journal of Economic Perspectives*, Spring 1994) had been read into this book's introduction or been fashioned into its conclusion. Of course, it helps to think about such value-sensitive behavioral puzzles via game theory, that is, as variants on the core problem of how best to promote cooperation or serve the common good in the context of a prisoner's dilemma. But translating even simple game-theoretic insights into workable institutional designs and policy decisions is itself a forbidding task that we might know how to perform better if more policy analysts paid more attention to values.

The book edited by Ingram and Smith is dedicated to the memory of the great Aaron Wildavsky, one of whose many seminal books was *Speaking Truth To Power* (1979). To begin to honor this biblical injunction, however, policy-oriented social scientists must first have truth to speak. That truth must encompass not only technical assessments of the efficiency or cost-effectiveness of competing policy options but broad-gauged understandings of how different options either nurture or negate democratic citizenship. Without insisting that "citizenship issues become the sole criteria by which policies should be judged," the editors and other contributors make a compelling case that "citizenship criteria should receive far more attention than they currently enjoy" (pp. 11–12).

Part 1 of the volume, "Policy Shapes and Citizenship," begins with Marc Landy's excellent discussion of modern antipoverty, environmental, and consumer legislation. In Landy's capable hands, these examples show that policy decentralization can empower citizens and enliven democracy but that it "must be limited to those policy spheres where minority rights can be fully protected" (p. 43). Landy breaks new conceptual ground by discussing policies as "constitutions" and their designers as "founders." He argues that "policymakers must reconcile specific policy objectives with a concern for the long-term political health of the Republic" (p. 43).

Also in part 1, Deborah Stone explores "clinical reasoning," defined as "a pattern of thinking derived from clinical medicine and used to evaluate and classify citizens in many areas of social policy far removed from health and medicine" (p. 45). She focuses on three areas: criminal justice, education, and the regulation of gender roles. She concludes that the "more clinicians define standards of healthy behavior—whether in learning styles, work roles, parental roles, or relations with others—the smaller the realm of deliberation and choice for individuals" (p. 65).

Part 2 of the volume, "Informing and Mobilizing Citizens," begins with Janet A. Weiss's nicely balanced discussion of the democratic pros and cons of a particular public policy instrument, *public information campaigns*, defined as "a government-directed and -sponsored effort to communicate to the mass public or a segment of the public in order to achieve a policy result" (p. 100). Part 3, "Public Policy for Empowering Citizens," concludes with H. Brinton Milward and Keith G. Provan's fine analysis of how the privatization of public services is neither self-executing nor necessarily a boon to citizen participation in the administrative process.

Part 4, "Public Policy for Reconnected Citizenship," consists of a single outstanding chapter by Richard M. Valelly. Valelly ponders various ways of "making political participation more individually meaningful for more Americans" (p. 242). Drawing deftly on everything from the works of Tocqueville and E. E. Schattschneider to relevant findings of contemporary political science, he uses the examples of environmental regulation and voter registration to argue that citizen involvement in group politics can and should be made easier.

Still, the question that hovers over this volume is whether its editors and some of its authors doth protest too loudly about the need, in Valelly's words, "to take public policy for democracy more seriously" (p. 264).

To cite just one example, in chapter 4, Ingram and Anne Schneider classify policies according to the characteristics of the populations who must alter their behavior if the policy is to work: *advantaged, contender, dependent*, or *deviant*. They worry that some citizens and their elected officials unfairly see other citizens as reckless, incompetent, or inferior—or worse. They hope that whatever such negative "social constructions" are embodied in subtle ways through policy design can be reduced or eliminated. One of their examples of a "policy directed at what is constructed as a deviant group" is mandatory random drug-and-alcohol testing for safety-sensitive transportation workers (pilots, motor carriers, railway engineers). "The public," they lament, "very much dislikes being subjected to risks, and target groups suspected of acting irresponsibly in matters of public transportation receive strong sanctions" (p. 83). In their view, the evidence linking substance abuse to transportation accidents is weak, and the 1991 federal laws subjecting transportation workers to various tests for substance abuse represent an "extraordinary invasion of personal rights" (p. 83). They would have designed the policy process in a way that would have prevented this ostensible abuse and frustrated public sentiment.

This leaves one with the uneasy feeling that when it comes to such issues as public safety, crime control, environmental regulation, or welfare reform, "public policy for democracy" ends where the policy preferences of civil libertarians, liberal policy analysts, and social democrats begin. A bit too often for comfort in this volume, the subtext seems to be that a good "policy design for democracy" is one that gives average citizens the right to be right but not to be untutored, misguided, mean-spirited, antidemocratic, or, worst of all, capable of effecting policy outcomes that are at odds with the contributors' own policy positions.

Still, whether taken together or separately, these books are well worth reading. If their basic messages about values and democracy are heeded, then a generation from now, public policy studies will be a less academically anemic, more democratically vital, less intellectually fragmented, and more politically consequential field than it is today.

Princeton University JOHN J. DIIULIO, JR.

Creating the Constitution: The Convention of 1787 and the First Congress. By Thornton Anderson. University Park: Penn State Press, 1994. 256p. $35.00 cloth, $14.95 paper.

Thornton Anderson's book is a provocative analysis of the drafting of the Constitution and how the governing document was interpreted during the First Congress. Anderson's thesis is that members of the Constitutional Convention who argued in favor of a strong national government often were dissatisfied with the outcome of debate and the Constitution's final form. Yet during the First Congress, they succeeded in construing the Constitution to enhance the central governments powers, in a manner contrary to the intent of the convention and to such an extent that if the actions of the First Congress had been foreseen, the Constitution would have been denied ratification.

Although the delegates to the Constitutional Convention are often divided into Federalists and anti-Federalists, Anderson suggests they were actually separated into three camps. Relying on factor analyses of voting at the convention, he classifies the delegates into the nationalists, the state Federalists, and those defending state sovereignty and equality. The nationalists included such obvious delegates as Madison and Hamilton; the state Federalist ranks consisted of such members as Pickney, Mason, and Sherman; and those advocating the primacy of the states included Martin, Paterson, and Yates. The state Federalists played a decisive role; for after the initial success of the nationalist Virginia plan, they allied themselves with those defending state sovereignty to reduce some of the powers of the proposed national government. Although the state Federalists rejected the state-egalitarians' New Jersey plan, they also became fearful of the nationalists' ambitions for the new government. Consequently, the great Connecticut plan came about as state Federalists joined with the state-egalitarians, forcing the nationalists to accept the compromise.

All three groups were motivated by reasons of politics and political philosophy, rather than by the Beardian view that economic gain animated the delegates. The ideas of limited government expounded by Sir Edward Coke and those of the Country Whigs in England, for example, provided the primary intellectual context for the convention; Lockean liberalism plays a minimal role in this interpretation. Anderson tests Beard by examining the debates on such economically connected issues as employing wealth as the basis for representation, property qualifications for office holders and voting, the use of bills of credit or paper money, the obligation of contracts, and the creation of corporations. Anderson concludes that although commercial and property considerations were evident, they "were clearly not often dominant" (p. 115).

Anderson is at his best when analyzing the First

Congress, where he explores how the nationalists reclaimed the initiative they had lost at the convention. With only a handful of anti-Federalists elected to Congress, the realignment of forces pitted the centralizing nationalists against state Federalists. Anderson demonstrates how, during the first session, the centralizers claimed that where the Constitution was silent on matters of federal authority, Congress could provide the necessary interpretation. In the second session, the centralizers moved beyond that point to exercise powers not expressly granted by the Constitution. In such victories as the assumption of the states' Revolutionary War debt and the chartering of the Bank of the United States, the centralizers' enhancement of the national government's authority outstripped not only what had been contemplated during the convention but also the Federalists' interpretation and defense of the Constitution during the ratification debates.

These successes, however, came at a high price. The adoption of Hamiltonian economic policies divided Washington's administration driving Jefferson and Madison into opposition and the nation into its first party system. Where the Southern congressional delegations were initially uncertain and disorganized in their response to the centralizers' policies, they soon became more aggressive and cohesive in their resistance, and the Congress increasingly polarized. Where the convention, guided by the state Federalists, provided for a system of "collaborative federal and state pursuit of consensus," the "bending" of the Constitution ultimately resulted in the Civil War (p. xv).

What is strikingly absent from Anderson's analysis is something more than a cursory examination of the events that intervened between the convention and the First Congress, namely, the publication of the *Federalist Papers* and the ratification debates. Anderson claims, for example, that the Constitution would have been denied ratification had the actions of the First Congress been known in advance. Were those members of the First Congress who "twisted" the Constitution reelected? If so, perhaps the public agreed with their views and the Constitution would have survived the ratification process. A rigorous comparison of how centralizing Federalist statements made during the ratification process conflicted with the activities of the First Congress would have enhanced Anderson's argument.

In addition, it is difficult to accept Anderson's broader conclusions that the First Congress resulted in the Civil War and "committed the country to a politics and a psychology of conflict" (p. 206). Anderson neglects to mention that from 1800 through 1860, the federal government was for the most part controlled by state Federalists in the guise of Jeffersonian Republicans, Jacksonian Democrats, and Democrats who willingly compromised on slavery and states' rights. Consequently, although some aspects of the federal government's centralizing powers were preserved, many others, such as Hamilton's economic system with its Bank of the United States, were not. The Southern state-egalitarians were also somewhat successful in restraining tariffs (e.g., the 1833 "Compromise" tariff). Ultimately, however, they could not win the numbers game that balanced free and slave states, which meant that even a Constitution administered by state Federalists could not contain the tensions inherent in the issue of slavery. Southern egalitarians recognized this to be the case, so that civil war was inevitable once the federal government returned to the control of the centralizers in Lincoln's election.

These comments aside, Anderson's well-written account provides a perceptive comparison of what was considered and produced at the Constitutional Convention with what was enacted at the First Congress. His categories provide a valuable analytical tool in the study of the founding period. Finally, Anderson's description of how the English Country Whig ideology influenced the convention helps to correct the notion that it had expired as an intellectual force in American political thought by 1787.

University of Virginia JAMES D. SAVAGE

Religion and the Racist Right: The Origins of the Christian Identity Movement. By Michael Barkun. Chapel Hill: University of North Carolina Press, 1994. 290p. $39.95 cloth, $15.95 paper.

During the 1980s, political scientists and sociologists debated the role of the Christian Right in American politics. Some saw the Christian Right as a threat to civil liberties, while others viewed the movement as mobilizing previously disenfranchised elements of society into political participation. While public attention was focused on the Christian Right, law enforcement agencies were far more concerned with a different, more dangerous set of organizations that also merged right-wing religion and politics. Groups such as the Order, the Aryan Nations, the White Aryan Resistance and others argued that the Zionist Occupation Government was illegitimate, and must fall. Some formed heavily armed survivalist enclaves, others committed murder and armed robbery.

Michael Barkun focuses his attention on the Christian Identity movement, tracing its origins, detailing its theology, and describing its political activities. If the Moral Majority never came close to claiming a majority, the Christian Identity movement is tiny: at one point in the book, Barkun cites estimates that range from 2,000 to 50,000, and at a later point he mentions a figure of 100,000. Yet Barkun suggests that we ignore the movement at our peril, in part because of its fervently apocalyptic vision and the potential for violent action by its members.

Christian Identity is characterized by three core beliefs. First, white Aryans are seen as descendants of the biblical tribes of Israel, or possibly as descendants from Adam and are the chosen people of God. Second, Jews are seen as the illegitimate spawn of a liaison between Eve and Satan in the Garden of Eden and therefore as a race that is inherently evil. Finally, the world is seen as on the verge of a final battle between the forces of good (white Aryans) and evil (Jews).

The first section of the book traces this belief to its origins in the British-Israelism movement of the nineteenth century. This earlier movement had posited that the British were at least some portion of the 10 lost tribes of Israel and that contemporary Jews were descended from the tribes of Judah. Although some in the movement voiced anti-Semitic sentiments, the official theology mandated the eventual reunion of the Jews with the British. Thus, far from demonizing the Jews, British-Israelism saw them as distant cousins who remained the chosen people of God. Barkun argues that this philosophy eventually evolved, in the United States, into a Christian Identity theology that paints the Jews as the spawn of Satan. He traces this evolution through the writings and speeches of a motley cast of characters with eccentric belief systems that include insights from the dimensions of the Egyptian pyramids, flawed linguistic analysis, and other unusual sources.

The second section of the book provides a fascinating exploration into the Christian Identity belief system. Barkun argues that the contemporary identity theology was the result of several specific doctrinal beliefs, several of which have rather long histories. He notes that Christian Identity differs from fundamentalism in its rejection of the "Rapture" in which Christ will gather the "saved" before the final days of tribulation. Christian Identity views anyone who wants to avoid the tribulation as wimps, because the final battle is to be welcomed. Yet their doctrine is curiously uncertain as to the outcome of this final battle, for the forces of Satan are described as quite formidable.

The final section focuses on the political activities of Christian Identity groups and activists. Because these closed enclaves eschew publicity, there is less detail here than we might like. He discusses the forays into the electoral arena and the extralegal activities, yet there is little here that did not appear in national newspapers.

Religion and the Racist Right is an invaluable introduction to the history, theology, and activities of a movement that has been generally ignored by scholars. It provides a fascinating exploration of a bizarre set of beliefs and their origins and consequences.

No book is without its flaws, however, and I was troubled by two aspects of this book. First, I don't think that Barkun provides an adequate explanation of the transformation of an idea that began as a belief that Jews and the British are both part of Israel and must reunite to one in which the Jews are the children of Satan and racially tainted with evil. Barkun argues that such an evolution was possible because the British-Israelism movement in Britain and in the United States never attempted to maintain any core doctrine but was a loose association of groups, allowing for doctrinal drift. Moreover, the rejection of conventional standards of valid knowledge allowed for various elements of the "cultic milieu" to enter. Finally, the core tenants of Christian Identity had existed in one form or another for some time, and so were readily available for combination.

Yet this seems to me to account for why the doctrine could drift but not why it changed so dramatically and in such a vicious direction. The British-Israeli movement appears to be the work of harmless eccentrics, while Barkun depicts an armed and dangerous Christian Identity movement. Barkun does suggest some additional elements of an explanation at various points of the book. For example, certain key individuals with the resources to build the movement in American happened to be anti-Semitic. The book also hints at the instability of a combination of beliefs that Jews are evil and that they are God's Chosen People. Of course, a complete explanation of the doctrinal change may not be possible.

In addition, I was somewhat troubled by Barkun's decision to keep his discussion of Christian Identity completely separate from ongoing scholarship on the "mainstream" Christian Right. Although Barkun argues persuasively that Christian Identity is very different from fundamentalism, some of the elements of this belief system—anti-Semitism, a belief that the United States is the new Israel—are not infrequently heard in fundamentalist circles. Moreover, while Barkun identifies David Duke as drawing Christian Identity support, it is also clear that Duke drew from fundamentalists as well. A brief discussion of the theoretical relationship of Christian Identity to larger, more conventional Christian Right activity might have been helpful.

Yet, overall, I found this book well written, informative, and thoughtful. It is carefully researched and documented (albeit with an annoying note style). I recommend it to students of the Right, of religion and politics, and of fringe social movements.

Georgetown University CLYDE WILCOX

Alternative Tracks: The Constitution of American Industrial Order, 1865–1917. By Gerald Berk. Baltimore: Johns Hopkins University Press, 1994. 243p. $35.95.

The Iron Horse and the Constitution: The Railroads and the Transformation of the Fourteenth Amendment. By Richard C. Cortner. Westport: Greenwood, 1993. 231p. $55.00.

While both of these books ostensibly deal with the genesis of railroad regulation in late nineteenth-century America, they are, in fact, very different. Moreover, it is fair to say that neither Berk nor Cortner is interested in railroad regulation in its own right, but, rather, each examines this subject as a means of exploring broader questions. Berk's book probes the political economic development of the United States, whereas Cortners' book delves into the evolution of substantive due process and judicial power. These excursions into finely nuanced historical (Berk) and jurisprudential (Cortner) analysis suggest the relevance of this formative American experience to our contemporary political environment. Berk, especially, is attuned to the potential meaning our contentious coming to terms with regulation at the turn of the last century has had for our transition into the twenty-first century.

Alternative Tracks represents an ambitious effort to make sense of how the modern American state was fashioned. In this regard, it falls squarely in the institutionalist political development tradition, associated most notably with Skowronek, though the analysis in this case is based on a single area of policy development, railroad regulation. The book presents a thesis that directly challenges the prevailing views of both business historians and political scientists. The former maintain that railroads in the United States developed as the archetypal case of huge, capital-intensive, managerially controlled firms, driven in their particular strategies and structures (as Alfred Chandler would put it) by the imperatives of technology and market forces. The latter argue that these new industrial behemoths came to be regulated through a political process in which various interests vied with one another, individually or in coalition, to produce a new set of regulatory institutions. Farmers, merchants, investment bankers, railroads, middlemen, sectional interests—all participated in the politics of group interaction. Berk rejects these versions of history, contending instead that (1) railroad development must be understood as a political outcome dictated far more by legislative and judicial intervention than technological imperatives and (2) our regulatory institutions emerged from a contest between competing ideological and philosophical conceptions of America.

It is on this latter point that *Alternative Tracks* makes an original and provocative contribution. Berk argues that the development of regulation, rather than a combat among competing interests, was really a contest between two competing public philosophies. One, *corporate liberalism,* is the well-understood politics of government mediating between the corporate and noncorporate sectors of the economy to redistribute wealth, thereby ensuring that corporate firms have the freedom to pursue their economic ends without trampling upon the rights and interests of the citizenry or small business. The other, which Berk introduces, is *regional republicanism,* a political economic vision of numerous moderately sized commercial and industrial centers bound together in regional economic networks and dedicated to the ideal of ensuring the modicum of equality essential to an independent, self-governing citizenry.

In a detailed yet accessible case study, he demonstrates that the Chicago Great Western Railway Company offers an example of an economically viable railroad based on a regional, short-haul strategy in the Upper Midwest. This case is intended to counter business historians' claim that the construction of interregional rail systems was a technologically determined outcome. In addition, he attempts to demonstrate that the Chicago Great Western's president, A. B. Stickney, was a leading proponent of a republican political economy, not simply a small rail operator pursuing his own interest.

If there is a problem in this analysis, it is that Berk makes a only a prima facie, rather than an truly convincing, case for the existence of a coherent ideology of regional republicanism. Citing Iowa senator James Wilson, he suggests that the regional republicans' case rested on the proposition that "no *privileged* person or community would lawfully connive to monopolize and control lines of transportation and trade in the United States" (p. 92, my emphasis). This quote could just as easily be used to argue that advocates of railroad regulation such as "Jefferson Jim" Wilson exemplified the innate revulsion at privilege that Tocqueville saw in American democracy 50 years earlier. Tocqueville also asserted that in democratic societies, the lack of intermediary institutions led inevitably to a reliance on the central government in times of socioeconomic insecurity. Thus, the movement for railroad regulation might well be explained as a popular response to the accumulation of privilege that Americans found intolerable and/or a popular demand for government intervention to bring stability in the face of economic and social dislocation. Viewed in this light, Justice Stephen Field's famous *Munn* v. *Illinois* dissent in which he decried regulation as tantamount to confiscation merely echoes the *Federalist,* in which Publius expresses chagrin at the "rage for paper money" and other popular vices. If this is so, railroad regulation is, as Berk argues, about considerably more than interest group politics, though recurrent episodes of egalitarian discontent, rather than regional republicanism, may be the touchstone of this period.

What is needed in Berk's essay is a more careful development of evidence for the widespread acceptance and influence of regional republicanism. More lengthy citations, instead of brief excerpts of speeches and writings, would have been especially helpful. While the concept of regional republicanism is stipulated and carefully defined, one is not quite sure that it was self-consciously adopted as a public philosophy by farmers, merchants, and their representatives. In addition, the translation of these ideas into policy proposals must be shown more clearly. The platforms of the Grange and merchants' associations that Berk asserts demonstrate that regional republicanism might just as easily be interpreted to embody alternative ideological views.

This difficulty notwithstanding, *Alternative Tracks* brings to light the fact that regulatory politics can provide a window on what Berk labels *constitutive politics,* that is, those epochal periods when Americans debate frankly our political economic relations and in doing so recast our Constitution. He is also insightful in noting that the feckless state of contemporary politics in America suggests we have entered a period analogous to the late 1800s and that we now have an opportunity to reopen our constitutional dialogue on property, citizenship, and freedom. It may well be that our era, like the Progressive period, is best characterized not by the politics of power but, rather, by a politics of redefining our public philosophy and institutions.

Cortner embarks on a somewhat less grand enterprise, retracing the arguments, pro and con, on the Granger cases of 1877 and explicating the litigation strategy through which the railroads persuaded the federal courts, by 1890, to reverse that decision and interpose themselves in state efforts to regulate railway tariffs. In the early portion of the book, we find the familiar story of railroad interests conflicting with farm interests, the organization of the Grange as an advocate for the latter, and the subsequent enactment of rate regulation in several states. The numerous railway challenges to these laws, of course, made their way to the Supreme Court and were rejected in *Munn* v. *Illinois.*

The remainder of the book charts the legal efforts to overturn *Munn* that culminated in the Iowa Freight Case and Minnesota Milk Rate Case. It is in these two cases that the doctrine of substantive due process was accepted by the Supreme Court, thereby providing a basis for federal judges to review state laws and Interstate Commerce Commission decisions on railroad rates. The grounds for such intervention, as Cortner lucidly shows, was the argument that rate regulation was tantamount to confiscation even if all legislative and administrative procedures were followed. The Court, by the 1890s, was unwilling to brook such "confiscatory" actions either from the state legislatures or the federal bureaucracy. In a fascinating story, we see how two railroad lawyers, William Goudy and John W. Cary conceived the strategy of pursuing the reversal of *Munn.* Moreover, Cortner demonstrates how the careful selection of test cases and the role of friendly, activist judges facilitated the attack on the Granger cases. In particular, federal circuit court judge David Brewer, a relative of Supreme Court justice Stephen Field, played a crucial role in developing the Iowa Freight Case and moving it along to the high court. Brewer also played a crucial role in adjudicating another landmark regulation case, *Ex parte Young,* in which the Court enjoined states from implementing unconstitutional regulatory laws. This strategy seems to anticipate uncannily the standard operating procedure of modern litigation practiced by social and civil rights advocates.

Indeed, Cortner's major claim is that the seeds of modern constitutional litigation and jurisprudence are to be found in the rate regulation cases of the late nineteenth century. Specifically, he asserts that we owe the nationalization First Amendment rights, as well as the right to privacy on which *Roe* v. *Wade* rests, to the substantive due process and national supremacy arguments worked out by Goudy and Cary in their attack on *Munn*. This is an interesting and ironic argument. Unfortunately, the evolutionary connection between the rate regulation cases and modern jurisprudence is simply asserted by pointing out analogies between the railroad cases on the one hand and civil rights, freedom of speech, and abortion cases on the other, noting that the 1925 *Gitlow* v. *New York* decision on freedom of speech provides a bridge between substantive due process applied to property rights and substantive due process applied to civil rights. The book would have been improved immeasurably if Cortner had employed the same care and attention to detail in making these connections as he did in telling the story of overturning *Munn*. The argument that railroad rate cases provided the foundation for contemporary constitutional law is confined to a five-page preface and a three-and-a-half-page epilogue. As it is, the reader is left with an intriguing but unsubstantiated argument.

Finally, one wonders why, if connections are to be drawn between the rate regulation cases and modern Supreme Court litigation, Cortner did not deal with the important recent decisions on regulatory takings. In these cases, activist, conservative judges appointed by President Ronald Reagan have aggressively looked for opportunities to apply essentially the same argument Justice Field made in his *Munn* dissent. The modern version is that social regulation, usually of the environmental sort, can amount to a seizure of property without due process. The revival of substantive due process attacks on regulatory laws not only poses a compelling historical analogy to the railroad cases but also seems an obvious cause for both scholarly and practical concern, in light of the fact that the federal judiciary, as Cortner himself points out, apparently abandoned this line of attack during the New Deal period.

Rutgers University, Camden RICHARD A. HARRIS

The State Roots of National Politics: Congress and the Tax Agenda, 1978–1986. By Michael B. Berkman. Pittsburgh: University of Pittsburgh Press, 1994. 195p. $49.95 cloth, $15.95 paper.

Public Opinion and Policy Leadership in the American States. By Phillip W. Roeder. Tuscaloosa: University of Alabama Press, 1994. 256p. $26.95.

Despite the telling signs revealing the limits of policy options in Washington, a focus on national political institutions and policies continues to dominate the study of American politics. This focus has addressed many important issues, but many questions have not and cannot be addressed by exclusive focus on national processes and institutions, even over time. The dynamic nature of federalism, the enhanced professionalism of state institutions, and the shifting arenas for many policy questions have elevated the substantive importance of state politics. As the states have grown in political significance, there has been a parallel growth in the quality and quantity of state politics research. Increasingly, scholars are looking to the subnational level for answers to a variety of important issues, such as the comparative impact of political parties (divided or unified control) on policy, the effects of institutional design on decision making, and the influence of differences in public opinion on policy. The books reviewed here focus on questions related to the analytical importance of states to a broader understanding of American politics.

What accounts for the monumental shift in tax priorities in Washington in the 1980s? According to the provocative thesis forwarded in Michael Berkman's *State Roots of National Politics*, the origins of national tax change may be traced to the state socialization to politics experienced by a growing proportion of Congress in the 1970s. This socialization was shaped in large measure by state policy responses that had emerged to cope with disparities in economic development among the states. Long the bastion of low-tax, laissez-faire approaches to economic development, the Sunbelt became a source of growing importance within the Republican party just as the region experienced significant economic growth. The industrial Midwest and Atlantic regions had suffered diminished growth as emergent industries, no longer tied to the physical advantages these regions held, moved to the lower-cost Sunbelt. Legislators in these states confronted regional economic decline by promoting tax-cutting, supply-side policies. In the 1970s, many state legislators moved up to Congress. The result was a shifting center of power in the Republican party to the West and a policy reorientation among many politicians within the Democratic party. Initiated in politics by the supply-side taxation debates occurring in many statehouses, this new breed of politician brought these experiences with them to Washington.

This is an important, well-written, and carefully reasoned study that takes us beyond the normal compartmentalization so characteristic of the vast majority of American politics studies. In a manner reminiscent of V. O. Key's efforts in *Southern Politics* (1949), Berkman asks us to consider important linkages between subnational politics and the national political agenda. In the process, he skillfully leads the reader through a host of topics, including economic development, federalism, tax policy, agenda setting, party change, generational replacement, political socialization, and political recruitment, among others.

While Berkman lays out a provocative argument about the influence of state experiences on specific national policy changes, there are fundamental differences in the constraints and opportunities that surround state and national fiscal policy that future research should explore more fully. In addition, the form of state economic interventionism of the late 1980s that he suspects might fuel future change in national politics has largely evaporated as states have returned to tax-cutting and incentive packages. As open economies, without control of their currency or migration, the states face persistent pressures from lateral competition from other states for capital and labor. Given these structural imperatives, it seems likely that the supply-side orientation will persist in the states. Record-breaking inducements offering huge tax breaks and incentives to lure industries have emerged in the past few years. These have occurred despite voluntary accords between states to end predatory competition for business. These strategies persist even when growing evidence suggests they do more harm than good. Over the long run, the supply-side orientation would appear to be a structurally induced predisposition at the subnational level.

I think it reasonable to ask if state experiences derived from the constrained fiscal environment of the states are suitable for national policy. This is particularly of interest when states have been abandoning investment spending in favor of tax cutting, apparently because of competitive pressures that are less operative at the national level. The question is even more significant in light of growing calls to "reinvent" government in a manner patterned after state experiences. This is not a flaw in the Berkman's excellent book, however, but a question that warrants future consideration as a follow-up to this path-breaking study.

In *Public Opinion and Policy Leadership in the American States,* Phillip Roeder uses the State Survey Project (SPP) of the Council of State Government/Martin School to assay public sentiment about several major public policy issues and political institutions from a state government perspective. Unlike past surveys that contained questions about American opinions concerning the most effective levels of government, the instruments contained in the SPP focus on citizen perceptions of state government institutions and public policy issues facing the states.

Over 20 years ago, Richard Hofferbert (1972) decried the lack of attention to public opinion in the states while acknowledging the practical problems that accounted for this neglect. He noted that collecting sufficient data to assess opinion about the states would require 50 times as many interviews as are included in national surveys. Despite these difficulties, there have been important studies of opinion in the states and the effect of opinion in the states on policies adopted by the states. Most recently, Erikson, Wright, and McIver have pooled national opinion data over time to overcome the small sample problem characteristic of past studies and make a strong case that opinions vary by state and state policies are highly responsive to state public opinion.

Roeder's work could be best categorized as an exploratory study of public opinion about state government and politics. Although following in the footsteps of some impressive previous studies, Roeder attempts to break new ground. Using the SPP, he seeks to assess citizen confidence in differing levels of government to tackle different social, economic, and political problems. He also considers the types of leadership people expect; their level of participation in local, state, and national affairs; and their opinions about increasing or decreasing federal power. Finally, using these data, Roeder explores how citizens feel about the responsiveness of various levels of government and the degree to which they prefer one level over another within policy areas. Having explored this previously uncharted territory, Roeder then presents and tests alternative explanations for differences he finds in public sentiments about levels of government.

What I found most interesting in the study was its depiction of the role states play as a significant, relatively distinct symbolic political predisposition. Remarkably, this predisposition is unrelated to ideology or party identification. The major shortcoming with the analysis is in the small sample problem noted. Roeder does not pool his samples; thus, state-specific questions cannot be addressed, due to inadequate samples from the states. Using single-year samples reduced many analyses to fewer than 700 cases. Roeder cautiously interprets what he concedes are often insignificant or vague findings. While such caution should be applauded, the reader cannot escape the feeling that some of the conclusions offered are highly tentative. If the three years of the SPP were pooled, this might provide sufficient observations to include state-specific controls. Such controls would likely improve the fit of many of the models presented in the study. With sufficient state observations, one could assess attitudes about level of government and policy as a function of the nature of specific state institutions, public policies, or economic conditions.

Each of these volumes illustrates the important conditioning influence the American states have on elite behavior on the one hand and on mass behavior on the other. Too often, courses in American politics ignore the subnational level. These courses take for granted or disregard many of the antecedent forces embodied in state-level politics that are critical to a full understanding of national-level political processes and outcomes. Undergraduate and graduate students of Congress, the states and policymaking in general have much to gain from Berkman's provocative study. Researchers in this area will also find many fertile ideas for future inquiry. Roeder's inquiry should stimulate discussion about the study of public opinion at the state level and will be a good choice for graduate courses on public opinion and state government.

Florida State University PAUL BRACE

The Mild Voice of Reason: Deliberative Democracy and American National Government. By Joseph M. Bessette. Chicago: University of Chicago Press, 1994. 289p. $32.50.

In *The Mild Voice of Reason* (a lovely title, from *Federalist* 42, by Madison) Joseph M. Bessette has set himself a daunting task. He seeks both to set forth a normative view of the proper place of deliberation in congressional policymaking and to demonstrate empirically that more serious deliberation than critics often acknowledge occurs in Congress. That he does not entirely meet either challenge reflects the controversiality of the philosophical issues and the complexity of the research problems he confronts. Still, he compels a serious reexamination of the conventional wisdoms that bargaining (logrolling, compromise, and the use of side-payments) and the lawmakers' pursuit of reelection are in themselves sufficient to account for the character of congressional policy outcomes.

Bessette's normative argument starts from his reading of the Framers' intent: They sought, he says, to establish a democracy that would be both "deliberative" and "energetic." Citizens would deliberate *through* their representatives, who would share the "values and goals" of the people (p. 2). The latter, insulated from domination by any majority faction or the whims of an "uninformed and unreflective" citizenry, "would be free to exercise some substantial independent judgment in making policy" (pp. 35, 38). The design of the congressional institution—small in size and with lengthy member terms of office and quality leaders indirectly elected—would permit policymaking in the public interest. The separation of powers, featuring an "energetic" executive and popular elections, would keep Congress from abusing its authority. "Three essential elements" characterize deliberative democracy, broadly defined as "reasoning on the merits of public policy": pertinent substantive *information* structured into *arguments* permits *persuasion* of the undecided or even opponents of particular policy initiatives (48–55). Deliberative democracy, then, will illuminate the content of policy and foster programs that advance the national interest.

This Madisonian view is essentially an elitist position that puts greater faith in the judgments of the senators and representatives than in the sentiments of the electorate. There are, Bessette concludes, two kinds of "public voice"—one "immediate or spontaneous, less well informed, and less reflective", the other "more deliberate, taking longer to develop, and resting on a fuller consideration of information and arguments; . . . only the latter is fit to rule" (p. 212). This stance, of course, raises normative issues that cannot be resolved definitively. Those skeptical of plebiscitary control of public policy will applaud the author's preference for a legislature that acts, within broad limits, as a trustee. They prefer a Congress that attends to public needs, rather than catering to the electorate's short-sighted or irrational wants. By contrast, democrats will opt for a legislature that is more responsive to popular preferences. They worry about a Congress that loses touch with those it is chosen to represent. They find attractive a legislature whose members are delegates and they look for ways (e.g., term limits) to yoke the lawmakers closely to citizen sentiments. They will not find Bessette's argument particularly appealing.

The author's empirical analysis is also problematic. To his credit, he does not argue that bargaining and the reelection motive are irrelevant. Rather, drawing on case studies of 29 bills, he suggests that congressional decision making (chap. 4) and the performance of individual legislators in pursuit of preferred policy (chap. 5) demonstrate substantial deliberation. While bargaining may influence a few (perhaps decisive) votes, "deliberative interpretations" better explain actions that look like bargaining (p. 74). Members have recourse to information, arguments, and persuasion in their efforts to get their colleagues to support, amend, or reject proposed legislation. Deliberation is more prominent in committees than on the floors. It occurs informally in legislative service organizations and unofficial congressional caucuses, private meetings, and contacts with outside groups and executive branch officials (chap. 6). Presidents promote deliberation when they, either publicly or in closed sessions with lawmakers, argue on the merits for items on their policy agendas (chap. 7). Overall, deliberation offers a more powerful explanation of congressional policymaking than bargaining or the electoral motive.

It is incontrovertible that legislators discuss substance in debating bills in committee and on the floor. Would one expect otherwise? What is less clear is the extent to which this is serious and influential deliberation. The best data—candid interviews with members—are unavailable. The case studies (the most recent being from 1971 as the book is a long-delayed revision of a doctoral dissertation) allow only speculation. Implicitly acknowledging the problem, Bessette often offers conditional if/then arguments: "If the majority report was an accurate reflection of the beliefs of most committee Democrats, then it is evident that they were persuaded by the case made at the hearings" (p. 89). If, however, the report was mere rationale for bargains previously made or electorally necessary actions, the story is quite different. Without hard data on legislator attitudes and intentions, it is difficult if not impossible to resolve the matter. The issue, then, is the direction of causality: Does the printed record, on which Bessette is forced to rely, reflect open-minded consideration of substantive questions or simply offer a gloss on positions assumed on the basis of other motives?

Bessette is surely correct to remind us that congressional policymaking is "a complex mix of politics *and* deliberation" (p. 151). The question is the degree to which the former or the latter predominates. The author argues for deliberation and Madisonians will hope he is right. The less sanguine will point to current conditions—policy gridlock and shrill partisan wrangling—to suggest that congressional deliberation falls considerably short of the author's claims. In the absence of recent and reliable data, the safest conclusion is to fall back on the old Scottish verdict, "Case for deliberation not proven."

Indiana University LEROY N. RIESELBACH

The Dynamics of American Politics: Approaches and Interpretations. Edited by Lawrence C. Dodd and Calvin Jillson. Boulder: Westview, 1994. 451p. $65.00 cloth, $24.95 paper.

The contributors to this collection of essays, originally presented at a conference at the University of Colorado, are intellectually ambitious. The study of American politics, the editors argue, has become so theoretically argumentative and divided over highly specific causal explanations of behavior as to make our understanding of the nation's political dynamics conflict-ridden, quite fragmented, and often confused. Is it possible, they ask, to develop a more consensual interpretive structure than we now have? They very much want to believe so. Their purpose, therefore, in this volume, is to answer the question Hugh Heclo frames in his concluding essay, "How are we to understand the changing shape of American politics? What approach or approaches hold out the greatest hope of yielding genuine understanding of change?" (p. 366).

To do this, the editors and authors engage, first, in a fundamentally didactic enterprise. They present a dialogue among proponents of different macroanalytic and microanalytic ways of thinking about American political dynamics in such a way that the particular strengths and weaknesses of each will be clearly demonstrated. Second, the editors hope that out of these forays, scholars will break free of their own compartments—talk and interact—and that from that some clearer, more consensual notions of how to explain both change and ongoing processes will emerge. Third, at the center of their efforts is their deep commitment to the study of America's political history with its patterns, contexts, regularities, and contingencies, which together offer "a unique opportunity for empirical discovery and theoretical learning" on these matters (p. 1). This last and central distinguishing characteristic of the volume is certainly no longer unusual. As David Brady and Elaine Swift suggest in their essay, "never before have political scientists been more interested in history," which they see as "an essential ingredient in building dynamic generalizations and accurate theory" (p. 83). I could not agree more.

The book opens with a customarily thoughtful and provoking foreword by Theodore Lowi. Sixteen essays follow, written by an all-star roster of 18 authors, alphabetically, in addition to the editors (who each contribute an essay), from John Aldrich, Brady and Walter Dean Burnham to Theda Skocpol, Stephen Skowronek, and Swift, along with the others involved, here, from Steinmo, Hanson and Greenberg, through Edelman, Huckfeldt, Beck and Stone, to Mansbridge and Karen Orren, all of whom have already established key explanatory and substantive beachheads in their own work. Each skillfully propounds a different explanatory nexus for understanding American political dynamics. Each's individual essay is a gem of delineation, clarification, and advocacy, adding up to a rich feast of insights, contrasts, and ways of understanding. The authors are all at the top of their form as they consider the best way(s) to analyze and understand America's political dynamics. Each expounds, follows through, and underscores the different ways scholars explain change, policy outcomes, successes, and failures. What determines systemic behavior, personal or social forces, values, culture or worldview, the state or the political economy? Is politics rationally driven, or socially, psychologically and symbolically determined? What, specifically, does historical data offer to theory building? Is the American experience exceptional? The ground is very well covered indeed.

One comes away from reading these essays, therefore, feeling as if one has been involved in a very intense and rewarding intellectual experience, impressed by the vigor, vitality, imagination, and learning of these scholars. But do the parts add up to the desired intellectual breakthrough? Is it, in fact, possible to come to some consensus about these many approaches and either anoint one as the most promising or find the right blend of several of them? The answer has to be that despite the book's extraordinary virtues, that part of the editors' purpose remains unfulfilled. While the authors do not exactly talk past one another, they are almost all committed to quite different approaches to explanation. As a result, despite the intelligence, knowledge, and openmindedness of each of them and their willingness to consider other perspectives, they do not move beyond clarification and their own prescription to delimit mutually marked pathways and understanding. There seems to be no clear way to overcome doubts and critical reservations so as to reach the desired intellectual nirvana, despite the valiant efforts by Hugh Heclo at the end to bring such order to the enterprise. These essays and approaches, he finally concludes, at best "offer successive and provisional approximations to partial knowledge" (p. 366).

There is a second matter. It is, of course, delightful and insightful of them that so many first-rate scholars are readily incorporating so much historical data in their work. But to add a little didacticism of my own, it is not clear to me, perusing the evidence and insights used and the citations listed, that all is as well as it might be. The historian in me wants to talk not about the well-established fragility and incompleteness of historical data (much of that can be overcome) but about the complexity of the historiographic record and the high potential for misunderstanding just where historians stand on critical points. That particular ground is often as contentious and as fragmented as the political science literature. How do we decide which historian to refer to and use? Which of their descriptions and generalizations are right? Which are more problematic? There are minefields out there in the historiographic record not always fully appreciated or readily overcome by visitors to it. Some of the contributors here fully understand this, have readily overcome the main epistemological problem, and are easy in their dealings with the record of the past. But there is room for more dialogue on such matters so that the record of the past is as clear and as useful as it can be made to be.

A final summarizing reaction to this exciting collection has to underscore both its achievement and its provisional quality, the stark presence of sharp disagreements, unsolved problems, ambiguity and uncertainty still. *The Dynamics of American Politics* is a superb work, a path-breaking series of critical epistemological dialogues, an excellent reference point for a wide range of notions and findings about political dynamics and change over time but not yet anywhere near a final version of the desired intellectual breakthrough.

Cornell University JOEL H. SILBEY

Statehouse Democracy. By Robert S. Erikson, Gerald C. Wright, and John P. McIver. New York: Cambridge University Press, 1993. 269p. $59.95 cloth, $18.95 paper.

This book brings to completion an ambitious project by Erikson, Wright, and McIver. The book integrates, refines, and elaborates their earlier work on partisanship and ideology across the states. This work presents numerous analyses that make a significant contribution to our understanding of state politics. It is a work that anyone interested in differences across the states should read carefully.

The project was apparently a reaction to the "determinants of state public policy" literature of the 1960s and 1970s, which concluded that socioeconomic conditions are the primary source of variations in state public policy. To assess the neglected role of public opinion, Erikson, Wright, and McIver assembled and merged the national CBS/*New York Times* polls from 1976–88 to create separate samples for each state. All these surveys asked respondents about their political ideology and their partisan identification.

This merged data set is used to construct a distribution for each state of partisan identification and ideology. These distributions are also converted into mean scores on ideology and partisanship for each state. The authors then create an index of liberalism of state policies and assess the relationship between ideology and public policy across the states. They find a strong relationship between ideology and the liberalism of public policy during the early 1980s. They also find that ideology (or opinion liberalism) has a much stronger relationship to policy liberalism than socioeconomic variables.

The conclusion is an important one for state politics. They find that the policies adopted across the states are strongly associated with the dominant opinions of the public across the states. The broad political process in the states does work to reflect and translate the opinions of the public into policy.

They also address several other important topics about political situations within the states. They assess the nature of political divisions within the states. They compute the mean ideology by party within each state and are able to assess the extent of mass polarization by state (p. 41). They also draw upon a previous analysis by Uslaner and Weber of state party elites to characterize the extent of liberalism by Democratic and Republican elites and the differences between the two sets of elites (p. 131). Their findings are of fundamental importance to state politics. We have lacked systematic information on the extent of political polarization and division within the states. Erikson, Wright, and McIver find that Republican masses and elites are more conservative than Democratic masses and elites in every state but that the degree of distance between them varies across the states. It has long been acknowledged that we need to have some sense of this polarization to understand party politics in the states. The efforts of Erikson, Wright, and McIver have finally given us our first comprehensive sense of these divisions at both mass and elite levels.

They also track changes in partisanship and ideology over time, using data sets from prior eras and find interesting shifts. One of the more interesting discussions revolves around changes in the role of fundamentalism. They argue that when issues revolved largely around economic opportunity, fundamentalism in the South translated into liberalism. As cultural issues came to dominate the national political agenda, fundamentalism produced a conservative reaction and pushed the South away from liberalism. In the 1930s, fundamentalism was positively associated with liberalism, but it now has a negative association with liberalism.

While the book makes very valuable contributions in charting ideology and partisanship differences across states, it falters when the analysis moves to using the data to assess the role of the political process. From my perspective, students of state politics have often been too willing to accept and use inadequate data and too willing to infer dynamics from static data. That unfortunately happens here.

With regard to the former problem, in moving to assess the impact of the political process on the connection between opinions and public policy, several problems emerge (pp. 113–49). Governors are excluded because of "measurement considerations" (p. 126). The percentage of the legislature held by Democrats is used as an indicator of Democratic legislative strength, even though we are all aware (and their data document) that the South is the highest on Democratic control, but the lowest on liberalism. Since there is a clear equivalence problem across states as to the meaning of party labels, it is unclear why such an indicator would be included in the analysis. In general, it is not clear that their broad, cross-section data on masses' and elites' attitudes are appropriate or valid indicators of the political process within states. We know nothing, for example, of the actual positions taken by gubernatorial and legislative wings of parties, *which* party controlled government, the negotiations between parties, and the effects of particular historical situations in each state. These broad validity questions are largely ignored. Gathering additional information would, of course, be a huge undertaking, and their efforts and accomplishments in this project are already considerable and commendable. The point is that it might have been more appropriate not to take such data beyond representing broad state differences to presume the data capture the political process. The authors' belief in their own data goes so far that they explain differences in their results from prior case-study time-series studies by arguing that prior analysts probably confused what they were finding (p. 118). Such a conclusion needs more substantiation. How the political process translates opinion into policy is clearly not a simple matter, and it becomes too simple here.

The second general problem that plagues the analysis of state politics has been that cross-section data are presumed to capture dynamics. This book contains discussions about the Downsian party model, the dynamics of party position taking, the interaction of mass reactions and elite adjustments, and the effects of policy enactment across time. While these theoretical approaches may or may not be of value, the data contained in this study do not capture such dynamics. We can only understand the dynamics of party position taking and elite reactions by examining actual histories within states. The movement to causal diagrams and discussions of dynamics is too uncritical here.

Despite these limitations, it is important to stress that this book makes some very significant contributions to our study of state politics. The authors are to be commended for undertaking this project, for the range of data they have assembled, and for contributing to our understanding of state differences. Finally, it should be noted that this is one of the better written books to appear lately. The writing is clear, concise and communicates very well. Graphs are used well. It is a model of clarity of presentation and writing.

Syracuse University JEFF STONECASH

The Argumentative Turn in Policy Analysis and Planning. Edited by Frank Fischer and John Forester. Durham: Duke University Press, 1993. 327p. $49.95 cloth, $19.95 paper.

This collection of essays is largely directed to faculty in schools of policy analysis, public administration, and planning. Among the recurring themes are (1) that successful policy analysis is not simply a matter of utilizing a set of techniques but also must include attention to making persuasive arguments; (2) that actors involved in policy disputes usually do not share a single frame of reference but, instead, adhere to competing belief systems that frame the way they see the world and what they accept as relevant, valid evidence; (3) that belief systems must be examined for their underlying assumptions and the actors whose power they legitimize; and (4) that policy analysts and planners should seek to foster more democratic decision making by helping ordinary citizens to understand the problems confronting them and the options available. To these authors, policy analysts who rely on techniques drawn from welfare economics and other technocratic forms of analysis are making at least two mistakes. First, they are ignoring the manifold problems with those techniques. Second, they are unlikely to be successful, because many actors will be operating from alternative frames of reference that do not accept the legitimacy of economic analysis (or perhaps even of science). Thus, planners and policy analysts must recast their role from one of providing efficient solutions to their clients' problems to that of helping people utilizing different frames of reference to understand those frames and to come to some agreement which is beneficial for the community as a whole.

I am sympathetic to many of these arguments. In addition, the editors and most of the authors in this volume are well-known scholars, including Martin Rein, Donald Schon, John Dryzek, William Dunn, Duncan MacRae, Robert Hoppe (University of Amsterdam), and Patsy Healey, one of the foremost contemporary scholars of English land-use planning. Many of these arguments can also be found in other recent works, including Deborah Stone's *Policy Paradox and Political Reason* (1988) and Giandomenico Majone's *Evidence, Argument, and Persuasion in the Policy Process* (1989).

Nevertheless, I find this book disappointing because it never presents a viable, coherent conception of what policy analysts should be doing and/or of their role in the policy process. Most of the arguments in this book (e.g., criticisms of welfare economics, the need to pay attention to the political context in which analysis occurs and to be persuasive to different audiences in presenting analyses, recognition of the ideological character of many policy disputes and even the belief-based assumptions of scientific paradigms, and the need for planners to pay special attention to the needs of politically disadvantaged groups) have been around for 20 years or more. By this time, we have a right to expect at least the outlines of a coherent alternative to the old paradigm (based upon the use of benefit-cost analysis and other techniques in politicized settings). Unfortunately, the "argumentative turn" has several important deficiencies and, until they are remedied, will represent a dead end.

First, while the editors and most of the authors are uncomfortable with epistemological relativism, they never present any substantive criteria for determining the validity of various frames of reference (belief systems). This is hardly a trivial problem: If all frames of reference are equally valid, how can one arrive at decisions in cases where multiple frames are present? Isn't this a prescription for either the status quo or for the raw use of power? John Dryzek discusses Laudan's criteria for dealing with conceptual and empirical problems in competing scientific research programs but rejects them because he doubts both the validity of science and its relevance to policy disputes. William Dunn presents an elaborate analysis of Toulmin's transactional mode of argument, but his discussion of "truth tests" strikes me as a classification of various tests, rather than an argument that some are superior to others (pp. 273–77).

Dryzek does, however, propose a procedural solution, *discursive democracy*, which draws heavily upon the "ideal speech" situation proposed by Jürgen Habermas. Since several other authors (particularly Patsy Healey and Bruce Jennings) present similar ideas and since most of the authors favorably quote Habermas, I shall assume that this is "the" solution offered by the authors (with the exception of Dunn and MacRae). It basically calls for unrestricted participation, the discussion of any and all topics, no resort to anything except argument, the equality of all participants, and decision by consensus (pp. 228–30).

In its pure form, discursive democracy is a prescription for the status quo ante—particularly in the case of "wicked" problems involving high technical uncertainty and high value conflict. Anybody talking about anything at any time with no minimum competency requirements and no deadlines is a prescription for wasting people's time and thus for either giving de facto priority to those who can afford to outlast the rest or to those who wish no change in the current situation. In a modified form (chiefly by restricting participation to the leaders of various stakeholders and usually by adding a skilled "facilitator"), discursive democracy may work in localized situations where the number of participants is limited, where participants share extensive knowledge of the local situation, and where they have social incentives to resolve their disagreements.

What about cases involving hundreds of thousands of people in urban areas, regions, states, and nations? Does discursive democracy offer a viable form of policy analysis? The book contains a very interesting case study of a successful effort in the state of Oregon to universalize medical coverage and to ration access to medical care. The key "actor" in this case was an 11-person commission composed of health professionals and citizens, which, in developing the priority list of health services, solicited public and professional input through surveys, hearings, and 47 town meetings. Although the author (Bruce Jennings) compares this process favorably to that of a purely legislative setting, he nevertheless complains that the commission was unrepresentative and probably overly swayed by technical expertise and elite opinion and that the town meetings, while open to everyone and well advertised, were attended mainly by health professionals and affluent, well-educated citizens (p. 112). If this laudatory effort did not meet the criteria of discursive democracy, what hope is there for social systems with larger, more diverse, and more dispersed populations than that of Oregon?

One of the fundamental problems with discursive democracy—at least as presented in this book—is that it assumes a model of the individual citizen that has no basis in reality: it assumes that most citizens are extremely interested in public policy, devote virtually unlimited time to it, seek information from a wide variety of sources representing different points of view, demonstrate great tolerance of people with opposing values, and somehow manage to engage in a rational dialogue with thousands of other people. It also assumes a small, simple society in which there is virtually no division of labor—in short, precisely the sort of society that is unlikely to generate policy problems, let alone wicked policy problems.

Another limitation with this collection of essays on the argumentative turn in policy analysis is that none of the chapters demonstrates any committment to fundamental norms of science, especially (1) a willingness to be clear enough to be proven wrong and (2) a commitment to intersubjectively reliable methods of data acquisition and analysis. Indeed, many of them view science as just another "frame of reference." This strikes me as unfortunate for at least two reasons. First, several of these authors make assertions about how the world works, but the evidence in support of their assertions is very incomplete and subjective. For example, Martin Hajer's chapter on the acid rain debate in Britain and Robert Hoppe's on changes in ethnicity policy in the Netherlands since World War II contain all sorts of assertions about the beliefs of various "discourse coalitions" over time but virtually no evidence about who believed what when (except for a couple references to commission reports). As such, they represent little more than interesting conjectures of unknown validity. Second, policy analysts usually deal with problems of some technical complexity, that is, cases in which the magnitude and extent of the problem (however defined), its causes, and the probable consequences of alternative solutions are unknown or in dispute. In such cases, scientific ways of knowing, for all their imperfections, are likely to be superior to other ways of understanding the overall situation, although the "ordinary knowledge" of practitioners and other actors in specific locales may also have substantial validity. In such situations, scientists from different coalitions are likely to disagree. But rather than just treating these as simply two actors with different frames of reference, policy analysts should seek to devise institutional arrangements to help resolve those disagreements according to professional/scientific norms.

University of California, Davis PAUL A. SABATIER

The Fatal Embrace: Jews and the State. By Benjamin Ginsberg. Chicago: University of Chicago Press, 1993. 286p. $22.00.

Put two Jews in a room, it is said, and an argument will soon rage among three opinions. In recent years, several prominent social scientists have undertaken major analyses of the political culture of American Jewry, and it should occasion no surprise that no two of them have reached the same conclusion. Now Benjamin Ginsberg, a political scientist heretofore best known as a prominent analyst of American political parties and public opinion, has joined the fray. In *The Fatal Embrace: Jews and the State*, subsubtitled *The Politics of Anti-Semitism in the United States*, Ginsberg sets out to identify the conditions that have historically given rise to Jewish success and to anti-Semitic attacks. He takes pains to emphasize that his purpose is to explain the conditions under which anti-Semitism is politically mobilized, not the conditions that give rise to anti-Semitism in the first place.

Ginsberg's basic point is that Jews have been closely allied with the state in the titular "embrace" that he argues has been consistently problematic and intermittently lethal. Following Hannah Arendt, he traces the rise and fall of the Jews to their relationship with the state. As a beleaguered minority, Jews have frequently sought the protection of the state. Moreover, as outsiders, Jews have also served as useful instruments for carrying out unpopular tasks and have come to play major roles in governance, albeit usually behind the scenes. A close identification with Jews can weaken the state, Ginsberg argues, and it can also serve as an invitation to political forces that seek to take over or destroy the established order to use anti-Semitism as a political weapon. The Jewish alliance with the state is, then, a two-edged sword in several senses.

Ginsberg writes knowledgeably about the experience of Jews in many societies and over the course of many centuries, but focuses primarily on the Jewish experience in nineteenth- and twentieth-century America. He portrays the middle half of the twentieth century as happy days for Jews, who were influential in American politics while anti-Semitism was relegated to the margins of American political life. In the years since then, he senses the reemergence of anti-Semitism, signs of which include the breakdown of the liberal coalition, an increase of anti-Semitism within the conservative coalition, and the return to prominence of extremist groups like the neo-Nazis and the Ku Klux Klan. The specter of a possible alliance between respectable conservatives and radical fringe groups haunts Ginsberg, for such an alliance "would almost inevitably make vigorous use of anti-Semitic themes to attack the liberal Democratic regime, and the Jews would find themselves locked in the fatal embrace of yet another state" (p. 243).

Statist explanations are in vogue, so Ginsberg's explanation of the periodic rise and demise of anti-Semitism will undoubtedly find a receptive audience. It deserves to, for this is an impressive treatment of an important and genuinely challenging subject matter. Especially noteworthy are the sheer depth of Ginsberg's historical treatment and the vast scope of his survey of the American case.

All the same, at certain points, Ginsberg's argument shows signs of strain, as he labors to fit data to his explanatory framework and vice versa. In part, this strain may be because the explanatory variable in Ginsberg's account (the "fatal embrace" of the Jews and the state) is, or is close to, a constant over time; but the phenomenon being explained (the periodic mobilization of anti-Semitic sentiments) is intermittent. This problem is not irresolvable; but resolving it would require a clearer, less ad hoc specification of conditions than Ginsberg has provided. I also tend to be more skeptical than he about whether the happy days of the 1950s were really all that happy for Jews in comparison to the 1980s and 1990s.

Ginsberg's chapters on the changing character of black–Jewish relations from the 1950s and 1960s to the 1980s and 1990s and the ill-fated Republican attempt to woo Jewish support during the 1970s and 1980s are informed and persuasive; but they seem rather far removed from his central theoretical argument. In contrast to his argument that attacks on the liberal state beget attacks on Jews, his treatment of the 1970s and 1980s keys on the Republican attempt to make political inroads among the Jews at the very time that the GOP was launching "a major offensive against . . . the domestic state that Jews had played such an important role in building and to which Jews continued to be closely linked" (p. 184). In this instance, at least, an attack on the liberal state coincided with a cozying up to Jews.

Finally, because of his explicit focus on anti-Semitism, Ginsberg devotes scant attention to what many analysts now view as a greater threat, assimilation. Some wag has remarked that these days Americans seem to express their negative feelings about Jews primarily by marrying them. Intermarriage, geographic mobility, religious secularism, and a host of related factors are combining to make Jewish Americans—never a cohesive unit—more diverse and less unified and identifiable than ever. Whether the "fire" of anti-Semitism or the "ice" of assimilation represents the greater threat to American Jewry remains to be seen.

George Washington University LEE SIGELMAN

Seducing America: How Television Charms the Modern Voter. By Roderick P. Hart. New York: Oxford University Press, 1994. 230p. $25.00.

This is a seductive book. Part polemic and part interpretive essay, *Seducing America* attempts to explain a package of changes that have decentered the American participatory experience over the last thirty years. Roderick Hart tackles a collection of political trends from the decline in voting to the loss of faith in government and politics, and from the rise of independent voters to the mean-spiritedness of civic life. This package is fetchingly wrapped up in the idea of "personalized politics." The seduction of the argument (and in my view, its over simplification) is in the claim that television is the causal force behind all of these changes. For those already inclined to see television as the national nemesis, the book's seduction proceeds from a simple premise: since television has mediated the recent national political experience, it is responsible for transforming that experience. For those who want to understand how or why television has assumed its distinctive role in American politics and in the psychology of voters, there is little satisfaction in hearing that television is how television feels and that it will always remain an egocentric and cynicism-inducing medium of communication.

Whenever the book escapes its preoccupation with television, Hart's considerable analytical skills offer many insights into the rhetoric of personalized politics. For example, a well-developed argument suggests that the intimacy of the televised experience invites people to think they really know candidates and historic moments as though they were insiders. This indeed, is the seduction of the theater, the novel, and the movie. However, unlike these drama forms which invite a knowing self-deception, contemporary political communication from the daily news to the campaign video biography invites people time and again to think they are somehow having a reliable experience. And time after time, the brute realization settles in that politicians prove to be unlike their advertised characters, and historic moments turn out to be far more complex and fraught with unanticipated consequences than they appeared on television. Hart contends that this constant experience of seduction and deception leaves citizens with a well-founded sense of cynicism about politics and politicians. These features of the modern experience are important and worth thinking about in the terms proposed by Hart. Another provocative discussion that runs through the book is a detailed account of the rhetoric of political intimacy with its characteristic focus on motives and morals over facts and public goals. These topics might well have been the focus of a most compelling book. Unfortunately, these and other insights all return insistently to the simple assertion that television is responsible for these forms of communication and for their presumed effects on disenchanted citizens. The unfortunate result is to turn television into something resembling a fetish, rather than offering a satisfying explanation.

This criticism does not ignore Hart's identification of the book as a polemic and a personal essay. However, the burden of argument remains with the author; and this argument is weakened by taking television largely as a given, a sort of late-twentieth-century monolith around which the political tribes have gathered to receive what passes for wisdom and experience. The reader must accept the premise that no causal forces operate upstream from the television signal. In Hart's bold view, "Television has changed politics itself" (p. 6). In particular, what has changed is how we feel about politics because of experiencing political reality through television. People have become alienated from each other and from organized politics because television personalizes the political experience, making each voter the center of the political universe and encouraging people to think of their self-interest above all as they make their political calculations. This is a tempting argument, but it would have been strengthened by at least considering alternative explanations for the turn toward a personalized politics.

To begin with, the "politics of the personal" seems to intersect in important ways with the self-interest ideology of the neoconservative politicians who have come to speak for large numbers of Americans in the recent period. What about the conservative swing in the political culture over the past 20 years? Did television beget the neoconserve rhetoric of the Christian Right, the Reagan revolution, and the Rush Limbaugh ditto heads? Or has an emerging conservative social movement simply used television to transmit a personally seductive message about political self-interest? There seems at least as much prima facie evidence for the latter proposition as for the former. And was it not television that brought America its first "mediagenic" president, Jack Kennedy, with his self-sacrificing exhortation, "Ask not what your country can do for you?" How do we account for two distinct periods of sociocentric and egocentric politics within the span of the television era?

Although the historical coincidence of neoconservatism and the politics of the personal would seem to merit greater consideration, Hart suggests that the kind of personalization he has in mind runs deeper than ideology, involving how people feel more generally about politics and politicians. However, even those aspects of personalized politics that seem to transcend ideology contain anomalies that challenge the television explanation. For example, by limiting the historical scope of the book to the period from 1945 to the present, Hart established an easy baseline comparison with Harry Truman, who surely would have fared poorly with the televised intimacy that seems required of successful leaders these days. But what are we to make of the fact that Truman also failed to communicate his leaderly qualities to the satisfaction of majorities during most of his pretelevision presidency? Even harder to ignore is the notable absence of any discussion of Franklin Roosevelt's pretelevision intimacy. Does Roosevelt's radio intimacy not count, for some reason? And how should we account for the implausible intimacy accorded to Andrew Jackson, one of the most taciturn and brooding characters ever to occupy the oval office? In John William Ward's account, *Andrew Jackson: Symbol for an Age* (1955), the quiet and distinctly nonoratorical champion of frontier democracy achieved a level of pretelevision (and largely fanciful) public intimacy that should make Ronald Reagan envious. Some effort to distinguish personalized politics in the pretelevision era from today's variety would have been useful.

Hart's insistence upon television as the cause of the national malaise constantly runs up against other equally compelling yet unexamined possibilities. Why, for example, should we concede the television spectacle as anything more than an expression of political discontents that emanate from other cultural, institutional, historical, or social conditions? Why should we assume that the American forms of political communication on television are anything more than artifacts of the many institutional arrangements that constrain (or fail to constrain) the political uses of the medium? There is no attention to the decline of party discipline, the rise of independent politicians, the political finance practices that generate huge sums of money for television advertising, or the weak American broadcast code regulations that affect the content and form of political communication on television. Have other societies with formidable television establishments experienced the same political decline as the United States? (Or, at least, have they declined in the same ways as the United States?) These and other important questions go a-begging. There is no discussion of television and politics outside the United States. Nor is there any discussion (other than dismissive) of the political and economic factors that shape the political uses of television inside the United States. The reader is encouraged to see such analytical concerns as somehow burdened by the "masculine" bias of standard political analysis and therefore insensitive to the feelings of the human beings in the televised society. True, the feelings evoked by watching television are identified as the focus of the book. Still, it is hard to avoid feeling that the resulting argument would have been strengthened by entertaining at least some critical challenges.

There is a hopeful moment following an insightful discussion of the Anita Hill–Clarence Thomas spectacle when the author asks the reader to remember that television "is an arbitrary art form. Nothing requires that television makes politics personal" (p. 49). Yet that idea is never followed up, leaving this reader to wonder how it found its way into the text at all. Indeed, any suggestion that television's deleterious effects can be ameliorated directly is firmly overruled by the book's pessimistic conclusion. Hart dismisses any hope for changing television because "television is what television is. It will not change fundamentally, no matter how many books are written to critique it, because its phenomenology is too seductive and its technologies are too entrenched. If change is to come, it must come from the commonwealth" (p. 171).

The model of change in the commonwealth that Hart proposes is provocative, taking the form of a New Puritanism to counteract the politics of personal indulgence. Indeed, the idea of promoting a self-critical, community-centered consciousness might come as a welcome antidote for personalized politics. Yet if Hart is right about television, how could such a political alternative ever receive a credible public airing? On the other hand, if television is more the medium for our national discontent than its cause, a more serious argument about how to stimulate cultural change would seem to be in order. However, exploring other origins of political consciousness would raise doubts about the importance of television, thus undermining the premise of the whole argument. Either way, the reductionistic use of television weakens the argument and hinders the development of many provocative ideas about rhetoric and culture that emerge in the course of this book, only to disappear again behind the veil of a seductive idea.

University of Washington W. LANCE BENNETT

Rethinking School Choice: Limits of the Market Metaphor. By Jeffrey R. Henig. Princeton: Princeton University Press, 1994. 277p. $24.95.

In the last year, at least four books have come out on educational choice. Jeffrey Henig's book is among the best of these, partly because it is more than a book about educational choice. It deserves a much wider audience than those drawn to issues of education policy. The book should be of interest to people in education, public policy and political science and economists with applied policy interests.

The book presents a well-thought-out challenge to educational reforms based on market-based theories of choice. The theme of the book is that the market metaphor cannot be realistically applied to some public goods, particularly education. The tone of the book is antichoice for programs that seek to incorporate private schools. The recommendation for reform in the final chapter suggests a limited role for educational choice as one of many tools available to public school districts.

The book is clearly organized in four parts containing nine chapters. Henig begins by "defining the problem." Chapter 1 provides an introduction, highlighting the recent importance of the choice issue and clearly outlining the argument of the book. Chapter 2 provides a background look at "the crisis" in American education. This chapter is refreshing and somewhat courageous. Most books of this ilk set up the problem by taking the simplistic position that American education is in serious decline of crisis proportion. Henig does not. He presents convincing data that in some regards, the system has and continues to produce educational successes. These include enrollment and school completion rates; improvements in academic credit taking; and positive public opinion data on American attitudes toward public schools. He also makes the point that often there is an automatic but inappropriate link between urban problems and perceptions of the performance of schools. His point is not that education is without problems but that ranting about crisis is more often than not a political means of setting up yet another call for radical reform—in this instance, by choice advocates.

Parts 2 and 3 review the evolution of the idea of educational choice and the history of choice programs and policies. The former focuses on the market analogy and the politics of choice, the latter, on the historical evidence concerning the success or failure of choice programs. In the chapters outlining the evolution of the choice idea, Henig adroitly combines an intellectual history that begins with Milton Friedman and ends with John Chubb and Terry Moe with a political history that goes back to choice as an escape from forced integration and ends with George Bush's GI bill for children in 1992. Although the tone of these chapters is antichoice and the main argument is that the market metaphor is misused, the treatment is relatively straightforward and evenhanded.

The basic thesis of the section reviewing the historical evidence on choice is that we know very little about the crucial issues, such as who would benefit from choice plans and with what effects on educational outcomes. In chapter 6, Henig appropriately criticizes many of the studies of both public and private school choice for failing to follow even the most basic research design principles, such as using control groups, doing longitudinal evaluations of the same students, and controlling for background and other factors that might affect both choosing and achievement. Thus, the argument that we know little is convincing.

Chapter 7, however, takes an odd stance, given chapter 6. Henig argues that choice has worked well when it involves strong leadership, public commitment, and solid political coalitions. Curiously, many of the projects that he earlier cited for inadequate research or data he now cites as successes. He then links success to factors that basically limit choice to public school choice programs. Choice plans outside this public framework are perceived as dangerous. The questions are, How, given the lack of good research, do we know that? and How does his prescription fit with his prior description of how in the early days of desegregation, other public leaders did everything they could to use choice to forestall integration?

Part 4, "Choosing Our Future," emphasizes the problems of implementation and the naivete of some choice reformers when it comes to administering and running choice programs. He appropriately stresses that the details of plans can make all the difference. He also focuses on the problems of turning the market metaphor rhetoric into a functioning program. Although the arguments and advice in this part are not new, they need to be constantly reiterated. They will probably only sink in when some state finally adopts a full-blown voucher program and then has to implement it in a short period of time.

Henig's final chapter contains two arguments. First, that choice should be encouraged in a circumscribed form as a tool for attacking educational problems. Second, relying on Clarence Stone's work, he points to the need to build "civic capacity" to provide the basis for sustained educational reforms. With this ending, he not only reinforces his argument that education is primarily a public and community activity but also that schools alone, with choice or not, cannot solve the education problems in our inner cities.

Rethinking School Choice not only offers a thought-provoking set of arguments but also a host of fresh ideas in many of the chapters. Henig is adept at bringing in literatures and ideas from other disciplines that refocus the way we look at education, and this goes well beyond the choice debate. The use of Deborah Stone's discussion of metaphors is one example; but others (e.g., the discussion of implementation in chapter 8) abound. In addition to making a major contribution to the educational choice and privatization debates, this book will force people interested in education and other policy fields to think in new ways about why and how policies originate, are advanced, and are implemented. That is not a trivial intellectual accomplishment.

University of Wisconsin, Madison JOHN F. WITTE

LBJ and Vietnam: A Different Kind of War. By George C. Herring. Austin: University of Texas Press, 1994. 228p. $29.95.

Although George Herring is a historian, this book is not simply a historical description of the Vietnam War during the Johnson presidency. Such an overview (to include other presidents) already exists in Herring's *America's Longest War* (1986). Drawing on diverse secondary literature and the archives of the Johnson administration, Herring aims to analyze LBJ's management of the American war effort, focusing heavily on how Johnson's leadership style contributed to his failures and successes as the nation's commander-in-chief and diplomatic leader.

The greatest strength of most chapters in this volume is their historical depth. Too many accounts of the Vietnam War fail, for example, to trace the influence of theories of limited war on policymakers in American national security bureaucracies of the 1950s and 1960s. Herring fills this gap persuasively, in my view, and effectively questions the assumptions on which such theories were built and then employed. Similarly, the challenges faced by Lyndon Johnson in dealing with the American military leadership are well placed in an evolutionary historical context. The American command system (e.g., the military, the presidency, and Congress, all functioning within a contested constitutional context) has functioned differently from one era to the next, says Herring, who quotes T. Harry Williams's description of this arrangement as "extemporized arrangements reflecting the national genius for improvisation" (p. 26). But even with the supposed rationalization of American foreign and defense policymaking by the National Security Act of 1947, which created the Defense Department, Central Intelligence Agency, and National Security Council, the system has never worked very smoothly. Well before the Vietnam era there were cases of near-open warfare between the presidency and the military leadership over defense policy. The Truman–MacArthur confrontation is well known, of course, but equally instructive for any student of national security policy is the Eisenhower era, in which a president insisted on limiting the military budget and watched with barely concealed fury as military leaders allied themselves with Congress to increase defense spending.

There is much in the Herring volume to recommend it. The chapters on the Johnson administration's attempts to pursue a peace settlement, to carry out "pacification" policies, and to manage American public opinion are carefully constructed, dispassionate, and sometimes insightful. Also, by extending his analysis to include the months after Johnson announced his forthcoming political retirement, Herring fills a continuing gap in the Vietnam decision-making literature.

For all the problems posed by fighting a limited war (which was probably unwinnable, Herring writes) and in spite of the predictable tensions within the American command system, Herring rates Lyndon Johnson a distinct failure as commander-in-chief. Early in the Johnson administration, the "frame of reference of discussions" about Vietnam was set by the military (p. 31). Later, with the war on, LBJ intruded into day-to-day tactical conduct of the war while failing to give a coherent, achievable strategic mission to military leaders. Johnson avoided the sort of searching debate that might have led him to avoid entering the war. After the United States was well into the conflict by the fall of 1965, he failed for two-and-a-half years afterward (until the spring of 1968) to engage in "any systematic discussion . . . of the fundamental issue of how the war was to be fought" (pp. 36–37).

There is much to respond to, critically, in these and other assertions. I will mention just three. First, Herring's claim that there was never any systematic discussion of how to fight the war, prior to 1968, seems unfounded. My difference with the author is based in part, no doubt, on divergent views of what counts as "systematic discussion." But as I wade through the increasing volume of newly declassified notes of advisory meetings in the Johnson White House, I am struck by the lengths to which Johnson went, on occasion, to bring in various parties to critical policymaking discussions in the White House. White House discussions about bombing policy in December 1965, for example, were extensive if not exhaustive. Certainly, in the spring and summer of 1967, the chairman of the Joint Chiefs of Staff, General Earle Wheeler, thought there was a painfully searching dialogue going on in the White House and Pentagon. ("A comprehensive review of U.S. strategy in southeast Asia" is how he described it to General Westmoreland in Vietnam.)

Second, while all scholars of the Johnson administration agree that LBJ usually sought to achieve a consensus in his administration over major foreign and domestic policies, political scientists and historians must carefully reconsider the conventional wisdom (mostly shared by Herring) that Johnson rigged his Vietnam (and other) policy processes to avoid critical debates about policy goals and means. I make this suggestion in light of the growing archival evidence casting doubt on that stereotype.

Third, there is the matter of Lyndon Johnson as a decision maker. LBJ hardly deserves to be ranked as a great commander-in-chief; but in all fairness, he exerted tremendous mental and physical energies, drawing on the talents of some of the smartest persons in American politics (including war critics), trying to understand the conditions in Vietnam and devise a winning strategy in the conflict there. That his strategy mostly failed does not mean that there was no strategy. Analysts attempting to evaluate a president's management of the Vietnam War must work very seriously to convey to readers the extent to which the "fog of war" extended from Vietnam to Washington bureaucracies and the White House itself, where policymakers reached sometimes differing conclusions and presented them to the president. In those chapters addressing this aspect of Johnson's performance, I believe *LBJ and Vietnam* does not consistently live up to the guideline set by James March and Herbert Simon in *Organizations* (1958)—namely, that social-scientific analysis of past decision making must carefully define situations as they appeared to the "rational actor" being studied, thus avoiding the pitfall of describing conditions in an organization (e.g., the White House) and its environment (e.g., the American, Vietnamese, and international arenas) with an "objectivity" from the privileged vantage point of after-the-fact observers.

Villanova University DAVID M. BARRETT

Less Than Meets the Eye: Foreign Policy Making and the Myth of the Assertive Congress. By Barbara Hinckley. Chicago: University of Chicago Press, 1994. 252p. $34.95 cloth, $13.95 paper.

In a book that repeatedly cuts beneath the surface, Barbara Hinckley explores the rivalry between Congress and the president in the realm of foreign policy. Her judgments are sharp, perceptive, and revealing. She takes us beyond the conventional assessments to probe deeply on the failures of both institutions. This compact book will serve as a healthy guide for future research.

Myth of the Assertive Congress in the subtitle refers to Congress's habit of regularly confronting the president in oblique manner, threatening a head-on collision with the president but, at the last minute, swerving away to leave hardly a dent. Congress performs these legislative maneuvers so often that they can be done almost blindfolded. Thus, it "prohibits" the president from taking a certain action unless he makes a certification that waives the restriction. Waivers are not hard to come by. Congress has it both ways. It dictates that a president abide by congressional policy unless he chooses not to. Congress can also register its discontent by calling for a report, prepared by the thousands without noticeable inconvenience to the president. Or it can resort to nonbinding legislative language, stating that it is the "sense of the Congress" or the "sense of the House" or the "sense of the Senate" that such-and-such is national policy. Presidents can regard these provisions as little more than congressional essays on public issues. Through them, the legislative body refuses to legislate.

The prime example of congressional huffing and puffing is the War Powers Resolution of 1973. Supposedly a statute designed to constrict the president, it widens executive power both in explicit procedures set forth and in the manner in which the statute has been implemented. Hinckley says that the resolution "restricts a president's independent authority to employ forces on a sustained basis to a sixty-day period (with a short extension possible), within which time Congress must vote either to validate or to invalidate the action" (pp. 84–85). But as the author later explains, the statute fails to restrict the president even to that degree because unless he reports under a specific procedure—Section 4(a)(1)—the clock never starts. President Ford was the only president to report under Section 4(a)(1), in the *Mayaguez* incident; but by the time he reported, the operation was over and the issue a nullity. Congress started the clock in 1983, for Lebanon; but instead of limiting the president to a 60-to-90-day period, it authorized military activity for 18 months.

Even the 60-to-90-day period is an extraordinary concession by Congress. Presidents are allowed to use military force anywhere in the world, for any reason, for up to three months without a shred of advance congressional authority. The Framers would have been stunned at the thought of granting a president that scope of power.

Hinckley describes another weak assertion of congressional authority—Members of Congress going to federal courts to complain that the president is violating the Constitution or the War Powers Resolution. On a regular basis, federal judges tell the legislators to go back to their institution and use the plentiful tools available to them. Hinckley has it right: "If Congress wants to confront the executive, it must do so by legislation and should not expect to be rescued by the courts" (p. 89).

To document her case, Hinckley collects statistics on bills, resolutions, funding bills, and substantive legislative (authorizing bills) to determine trends in legislative activity. Attempts to quantify legislative activity are always welcome, but they need to be supplemented by other measures. For example, the author does not discuss the very important "nonstatutory" techniques for controlling the executive branch. For a variety of reasons, executive officials come to congressional committees to seek approval on reprogrammings, transfers, reorganizations, and other agency actions. This delicate, informal system merits close attention. The author also excludes "resolutions" without making clear whether she means *joint resolutions*, which have the same legal effect as a bill passed by both Houses and submitted to the president.

Hinckley is critical of the compromised intelligence authorization bill in 1991, which incorporates reforms from the Iran-Contra hearings. The bill, however, contains a number of significant restrictions on future presidential activity. She objects also to the fast-track procedure used for trade bills, correctly pointing out how they handcuff Congress. But the whole theory behind fast-track is that it allows members of Congress to become deeply involved in the *negotiation* of trade agreements, a substantial concession by the executive branch and a point the author does not make. Fast-track is a classic quid pro quo.

In discussing the Gulf War debate in 1991, Hinckley faults Congress for passing legislation authorizing the war without ever "declaring" it (p. 93). Why, she asks, did Congress announce the "moral equivalent" of a declaration of war instead of following the constitutional procedure? That makes it sound like Congress dodged a responsibility. However, the Framers understood that a declaration of war is not constitutionally required. For example, the quasi war with France in 1798–1800 was authorized by Congress, not declared.

These are small points in an otherwise rich and thoughtful book. Hinckley is on the money in one section after another, such as when she describes the "deceive-and-forgive" pattern of Central Intelligence Agency director Casey's regularly misleading Congress, being reprimanded and then forgiven, and returning to the same deceitful practices. The tiresome cycle of legislative wrist slapping and executive apologies does credit to neither branch. The book ends by rejecting five potential areas of reform (I agree with her analysis) and recommending more constructive avenues of change. Readers will find much that is stimulating in this book, both for what it contains and what it will inspire in subsequent studies. We need to rivet our attention. The current institutional weaknesses in foreign affairs and the war power are ominous.

Congressional Research Service LOUIS FISHER

The Presidency in a Separated System. By Charles O. Jones. Washington: Brookings, 1994. 338p. $39.95 cloth, $16.95 paper.

Charles O. Jones is a wise scholar; and his new book, *The Presidency in a Separated System*, although not flawless is a work of wisdom. Increasingly in recent years—and in tandem with their hope of emulating the natural sciences and the more scientifically ambitious social sciences, especially economics and psychology—political scientists have come to regard what Ernest Boyer has called the "scholarship of discovery" (i.e., of new findings) as the only scholarship of value. Wisdom and its related virtues—insight, interpretation, synthesis, understanding—are not what such scholarship is about. Instead, wisdom's sphere is what Boyer calls the "scholarship of integration," the kind of scholarship that takes the findings of many others and makes sense of them. One way to summarize the difference between the scholarship of discovery and the scholarship of integration, Boyer writes, is this: "Those engaged in discovery ask, 'What is to be known, what is to be found?' Those engaged in integration ask, 'What do the findings mean?' "

Jones's book fits securely into the integration category. He draws extensively on the work of others, classic studies of American politics such as Chamberlain's *President, Congress, and Legislation* (1946) and Neustadt's *Presidential Power* (1960) and such modern studies Edwards's *At the Margins* (1989), Peterson's *Legislating Together* (1990), Bond and Fleisher's *President in the Legislative Arena* (1990), Hargrove and Nelson's *Presidents, Politics, and Policy* (1984), and, above all, Mayhew's *Divided We Govern* (1990). But Jones does more than summarize these and other books. He forges a creative synthesis out of them that allows him to offer a new way of understanding the familiar subject of the presidency.

Jones's thesis is that no single model (especially, the prevailing "presidency-centered, responsible-party perspective," [p. xii]) is adequate to describe either the presidency or its place in the political system. Instead, he urges that we look on the presidency as part of a system of appropriately "separated institutions competing for shares of powers" and thus an intensely variable office (p. 207). One practical effect of such an approach is to reclassify divided government as one of many flowers that the Constitution allows to bloom, rather than as a weed to be yanked out of the garden.

The theme of variability in the presidency colors the organization, as well as the contents, of *The Presidency in a Separated System*: "There have been ten post–World War II presidents, but these ten have had many more presidencies" (p. 24) "Presidents will enter the White House with variable personal, political, and policy advantages or resources," writes Jones. White House and cabinet organization are "quite personal in nature." Public support is "an elusive variable." "Lawmaking will vary substantially. . . . The challenge is to comprehend the variable role of the president." And Universal reforms are discouraged by a Constitution that allows for different types of governments." (pp. 24–25, 290). In the course of treating these topics, Jones returns frequently and at great length to the experiences of every president from Truman to Bush. There are minihistories of the post–World War II presidency, presidents, and domestic public policy imbedded in this book.

All students of American politics, not just presidential scholars, will want to read what Jones has written. Public opinion polls "do not change the basic structure by which presidents govern" (p. 116). Again, "The dominant models—the hierarchy or chief of staff and the circle or spokes of the wheel—vastly oversimplify the many differences among presidents' interactions with staff" (p. 101). Again, "There are very few new initiatives, and virtually no new programs originate, live, and die within one presidential administration" (p. 184). Again, "The grade given a president [for cabinet organization] properly may be based on how many officials meet the one-on-one test of contact and communication on issues of the moment, not on tests derived from communal decision-making" (p. 61).

One example of how Jones's broad awareness that the presidency is a highly variable institution is helpful is his analysis of the distinctive challenges faced by vice-presidents who succeed to the presidency—around a fourth of all presidents. None of the postwar presidential successors—Truman, Johnson, and Ford—could have been nominated for president in his own right, Jones points out; yet all had had much longer prepresidential political careers than the typical elected president. Each of them inherited an administration and a policy agenda not of his own making; each, too, lacked the experience of testing his own ideas on the campaign trail before entering the White House. Curiously, however (although Jones does not mention this fact), in stark contrast to their nineteenth-century predecessors, all five of the twentieth-century presidential successors except Ford (who came close) have gone on to reelection and, as a group, have been more successful, in the eyes of historians, than the century's elected presidents.

To praise Jones's book for its wisdom is not to say that it is unflawed. His preface bespeaks an impatience—even a testiness—toward those who continue to find merit in the presidency-centered model. Yet the most extended analytical section of his book makes one wonder whether it is he, more than those whom he criticizes, who has overstated his argument.

Here is what I mean. As a way of testing the usefulness of the presidency-centered model, Jones offers, in chapters 6 and 7, 28 case studies of important laws enacted from 1946 to 1990. He then classifies each case into one of four categories: preponderant presidential influence in enacting the law, preponderant congressional influence, joint influence but with active involvement by the president, and "true balance" between congressional and presidential influence.

Conceding that "someone else engaging in the same exercise would produce somewhat different results" (p. 208), Jones concludes that only 6 of his 28 cases are "president-preponderant." Such a conclusion is grist for his mill, of course. To argue that the presidency is a highly variable institution is to imply that presidents are dominant actors sometimes but not usually.

Well, maybe. When I read Jones's case studies, I concluded differently: relying on the evidence he provides, I would classify not 6 but 18 (well over half) of his cases as exemplifying presidential preponderance. The reason for Jones's low figure is his apparent assumption that if even some members of Congress are involved in an issue before the president steps forward (e.g., Medicare, the Department of Education) or if Congress as a whole does anything more than rubber-stamp a president's proposal (e.g., Manpower Development, Occupational Safety and Health Act), then the president has not been preponderant. I do not see it that way; nor, I think, do many of the "presidency-centered, responsible party" people that Jones excoriates.

Further support for a more presidency-centered model than Jones prefers (but a less presidency-centered one than he caricatures) almost certainly would be found in a study that included two areas of presidential activity that Jones decided not to emphasize in this book—foreign policy and the president's relations with the bureaucracy and the courts.

As Jones says, reasonable people may differ. But my guess is that even those who differ with him on particular aspects of his analysis will find it unreasonable to disagree with the excellence—nay, wisdom—of his book.

Rhodes College MICHAEL NELSON

The Supreme Court and Constitutional Theory, 1953–1993. By Ronald Kahn. Lawrence: University Press of Kansas, 1994. 320p. $35.00.

The comedian Woody Allen observes that "relationships are like sharks: they either move forward or they die." The same is true for scholars of constitutional theory and the Supreme Court: there are no good insights, only new insights. Ronald Kahn's provocative book fits this model of creative reinterpretation as it boldly challenges students of the modern Court to take constitutional theory seriously and to heed the power of institutional forms in the decision-making process. Kahn's objective is nothing less than a wholesale revision of our understanding of the Supreme Court's pattern of behavior from the Warren era to the present. Kahn attacks the notion, made most forcefully by Victor Blasi in *The Burger Court: The Counter-Revolution That Wasn't* (1983), that the Burger Court failed to reverse the tide of liberal justice unleashed during the Warren era. According to Kahn, the Warren Court majority was far less concerned with equal protection issues in the social sense and much more in the political sense—denial of the right to vote, for example—than previous scholars have recognized. Given the opportunity to grapple with the complex matter of informal discrimination and inequality, the supposedly liberal Warren Court demurred, leaving Justice Burger and his colleagues to do most of the heavy lifting with regard to rights. There was no counter-revolution, Kahn concludes, because there was no revolution to counter. Those scholars who have argued that there was have distorted history, bequeathed a false understanding of the Court, and nurtured an undeserved skepticism about its operations.

Few modern liberal scholars of the Supreme Court escape Kahn's revisionist gaze. On the one hand, he criticizes the instrumentalists, such as Martin Shapiro, Robert Dahl, and Bernard Schwartz, for over emphasizing the political concerns of the justices and the ability of political actors to shape their behavior. On the other hand, Kahn chides the so-called civic republicans, represented by Michael Perry, Mark Tushnet, and Sanford Levinson. These scholars, Kahn claims, have "introduced a vision of polity far less complex than is warranted by polity problematics in our Constitution and the social/economic structures in society" (p. 212). Kahn finds the civic republican interpretation simple-minded because it fails to appreciate that politics and rights are always at odds with one another. The civic republicans, Kahn claims, have done constitutional interpretation a major disservice by arguing the impossibility of maintaining a critical tension between polity and rights values in the Constitution. Only conservative scholars, such as Raoul Berger and Garry McDowell, escape attention, perhaps because, given the book's overall thesis, Kahn finds much to embrace in their work without actually saying so.

Kahn's argument holds that the Supreme Court is best understood as a constitutive rather than a political institution, one that gives great weigh to the idea that its rulings are far more autonomous than either instrumentalists or civic republicans appreciate. The constitutive approach, according to Kahn, properly stresses the Court's legal nature and emphasizes the place of developing polity and rights principles, precedent, and constitutional theory as significant to "constituting" constitutional law and principles. Contrary to the notions of realistic jurisprudence, Kahn stresses that a close reading of the Court's work demonstrates that at crucial times the justices have made decisions that are in conflict with their personal policy preferences and those of the presidents that select them.

Why do the justices act this way? Because, Kahn concludes, the justices "view the Court as a countermajoritarian legal institution and share a concern for the legitimacy of the Supreme Court and the rule of law" (p. 4). The justices see their jobs, Kahn continues, not as policy makers, but instead as *jurists* who seek to develop polity and rights principles in a dialogue with the interpretive community that follows the Court, most notably legal scholars and political scientists and theorists. It is these individuals, and not political actors or voters, that form the justices' most influential constituents. He wisely reminds us that constitutional scruples and a concern with process have figured heavily in the work of the Court.

Kahn continues the current trend of taking ideas seriously when explaining constitutional scholarship, of finding in patterns of thought and belief an explanation for judicial behavior, and of reminding us that Supreme Court justices do fret about their intellectual integrity in the community that has as its business interpretation of the Constitution. Save for the most originalist of justices, Kahn notes that the justices understand that they must be prepared to learn about and embrace changes in rights. In reaching this conclusion, Kahn offers something for conservative commentators (who argue that precedent and legality and not politics should guide the high court) as well as liberals (who stress the Court's important role in adjusting public law to changing social and political realities).

Scholars of the Court will find Kahn's revisionist theorizing riveting but his historical sensibilities puzzling. He is oddly willing to take the justices at their public word, to treat high court opinions as works of theory, and to build his interpretation without turning either to the justices' papers or their published memoirs. Kahn's sweeping argument depends for its authority, as does much constitutional theorizing, on the author's logical assertions rather than a broad evidentiary base. Kahn's theory, for example, places him in the uncomfortable position of attempting to make a constitutional scholar of Chief Justice Warren, perhaps the least scholarly of any chief justice in American history. While there is probably no requirement for Kahn to have consulted the papers of Earl Warren and several of his brethren in the Library of Congress, it does not seem too much to ask that Kahn dip into Warren's *Memoirs* (1977). Had he done so, he would have found, as have Warren's biographers, that doing justice was more important to the "Super Chief" than constitutional niceties. He would also have discovered that Warren disparaged the interpretive community that Kahn views as so central to understanding the Court. Warren, after all, wrote in his *Memoirs* that he had little patience with the concepts of strict construction and neutral principles, describing the latter as "a fantasy . . . used more to avoid responsibilities than to meet them" (p. 333). Warren knew that he had to make choices, that those choices shaped public policy, and that shaping public policy, especially in offering protection for the underdog, was why he was on the bench. Kahn's constitutive interpretation, while carefully nuanced, falls short because in focusing so fully on constitutional theory and jurisprudence it drains Warren and the other members of the Court of their life's blood—the courage of *their* convictions. We are left, therefore, with Woody Allen's shark, moving forward in a scholarly world in which there are no good insights, only new ones.

Ohio State University KERMIT L. HALL

The Capacity for Wonder: Preserving National Parks. By William R. Lowry. Washington: Brookings, 1994. 220p. $28.95.

"Today, decisions are made for politics instead of for the resource." William Lowry uses this quote from Yosemite superintendent Mike Finley to open the first chapter of his fine book, *The Capacity for Wonder: Preserving National Parks.* The remainder of the book is a detailed effort to answer the related questions of how this situation developed, what difference it makes, and how decision making in the National Park Service might be different.

The central question of this book is why the Canadian Parks Service has been, over the past 15 years, more successful at preserving national parks than has the National Park Service in the United States. Lowry draws upon the literature in public administration, political science, and economics to account for an agency's ability to pursue goals—in this case, the goal of ecosystem preservation. For Lowry, the ability of an agency to pursue a goal is a function of the consensus behind the goal, the level of political support for the agency, and the commitment of agency personnel to the goal (a constant in the two park services). Specifically, deteriorating goal consensus and political support alter the operating environment of agencies, and changes in the operating environment alter both the process and substance of agency policy decisions.

The author draws his data from an impressive compilation of documents (both public and internal) and over a hundred interviews with park officials in both countries. Through these interviews, Lowry does an excellent job of tapping into the experiences and perspectives of rangers, park superintendents, and regional park administrators. Conspicuous by their absence, however, are interviews with upper-level political appointees or legislative overseers in either country. Finally, the book presents a wealth of descriptive data on park funding levels, visitor pressure, commercial development, and the like. Readers looking for rigorous quantitative evaluations of parks policy will be disappointed; the only statistical analyses in the book are two rather incidental ordinary least square models of funding for individual park units in the two countries. Still, Lowry does a fine job in using the data available to craft solid qualitative tests of his hypotheses, going so far as to evaluate alternative explanations for observed changes in agency behavior.

One strength of this book is theory: it has one, unlike much of the more recent work in national parks and public lands policy. A second strength of Lowry's analysis is that it is comprehensive, following the influence of politics and the characteristics of the political system through administrative structure and into the substance of policy decisions. A third strength is the use of the comparative research design. Contrasting the National Park Service with the Canadian Park Service works wonderfully as a strategy to isolate and test variables hypothesized to affect agency behavior, while providing students and scholars of American politics with an illuminating substantive counterpoint to their domestic experiences. This strategy also presents a fine example of the value of qualitative research. Finally, Lowry concludes his book with a list of recommendations for improving the administration of national parks, particularly in the United States. While a couple of these recommendations face significant political obstacles (e.g., calling for new enabling legislation for the National Park Service), most are politically realistic and reflecting the most recent research in effective public administration (e.g., an increased emphasis on public education and communication in implementation).

Ironically, the book's most significant weakness is related to one of its strengths—its theoretical contribution. Lowry's work is theoretically strong when compared to other books on national parks policy and administration. On the other hand, the book has a fairly weak, almost casual presentation of its theoretical contribution when compared with the best work in public administration and bureaucratic politics. This weakness manifests itself in two ways. First, by focusing on how agencies respond to political direction and intervention, Lowry's story is really about the effects of efforts at political control of the bureaucracy. With the exception of Moe's chapter, "The Politics of Bureaucratic Structure" (1989), however, references to standard works in this area by Calvert, McCubbins, Moe, Moran, Weingast, Wood, and others are completely absent. Lowry provides a much-needed extension and revision of the arguments presented in this literature, but it's not clear that he is aware of this. Second, Lowry makes a half-hearted effort to show how the primary factors of goal consensus and political support might affect the behavior of other public bureaucracies. These two oversights—ignoring the literature on political control of the bureaucracy and a weak effort to extend the theoretical framework to other agencies—mean that *The Capacity for Wonder* misses an opportunity to contribute to building a cumulative theory of agency behavior.

A second, less serious shortcoming is the failure to assess the validity of competing goals for national parks policy. While a good deal of space is devoted to discussing the goal of preservation and the dual mission of the National Park Service (i.e., preservation vs. use), a strong argument is never made for why preservation ought to be the primary goal of national park policy. This shortcoming has two important implications. First, Lowry describes the decentralized, discretion-driven, preservation orientation of the Canadian Park Service as a "smart" and "intelligent" approach to managing national parks. The Canadian approach is neither smart nor intelligent, however, if (1) responsiveness to elected officials is the primary responsibility of bureaucrats or (2) we place a high value on economic rationality. Judged by these criteria, maybe it makes sense to let air quality deteriorate in Shenandoah National Park, maximize vehicular traffic in Yosemite, or even to build "Steamtown." There are solid and important arguments against these positions. Indeed, Lowry's analysis suggests that being "smart" and being "rational" are not always the same thing. These arguments are not presented, however; nor is the relevant literature consulted. By not addressing explicitly these arguments for a less preservationist approach to national parks, Lowry runs the risk of preaching to the choir and having less of an impact on the policy debate surrounding the national parks (and Lowry clearly wants to affect the terms of this debate).

A final, quite minor criticism is that from time to time, I would have appreciated more rigorous documentation of evidence. Granted, much of the evidence presented comes in the form of vignettes from the author's personal experience; and excessive documentation might detract from the casual, readable style that characterizes much of the book. Still, some readers might find relying upon bibliographic essays to document major themes to be distracting.

These shortcomings notwithstanding, this is a fine, well-written, well-researched book. Lowry presents a superior theoretical and substantive understanding of national parks policy than William Everhart's *National Park Service* (1983), John Freemuth's *Islands under Siege* (1991), or David Simon's *Our Common Lands* (1988). Only Alston Chase's *Playing God in Yellowstone* (1986) rivals Lowry's detailed research and understanding of politics, but this information in Lowry's book is more accessible and broader in scope. Outside of the subfield of environmental and public lands policy, Lowry's book teaches us volumes about the substantive policy implications of the political direction of agency behavior and should be of tremendous interest to students of bureaucratic politics and administration.

Florida State University EVAN J. RINGQUIST

Rights at Work: Pay Equity Reform and the Politics of Legal Mobilization. By Michael W. McCann. Chicago: University of Chicago Press, 1994. 372p. $65.00 cloth, $18.95 paper.

Michael McCann's *Rights at Work* is a book of broad scope and great theoretical sophistication. It examines the origins, development, and accomplishments of the movement for pay equity in the United States and its use of the legal system. The Pay Equity movement maintains that job categories that have traditionally been dominated by women or minorities remain undercompensated when compared with similar "male" jobs because of historic social and economic biases against these groups. Equity activists urge systematic evaluation of the worth of job categories in terms of skill, training, responsibility, experience, and general working conditions to remedy this inequity. McCann presents us with examinations of the national—and many of the state and local—battles over raising the wages of these workers. Building on Stuart Scheingold's classic contention in *The Politics of Rights* (1974) that although the law has significant limitations as a mechanism for delivering social change, it can be a powerful conferrer of entitlement and legitimacy, allowing the mobilization of groups challenging the status quo, *Rights at Work* goes still further. It holds that the Pay Equity movement derived substantial benefit from use of legal tactics despite its limited success in the courts.

On its face, this is an intriguing proposition, since use of the legal system is costly and social movements, by definition, represent interests that are excluded from normal "mainstream" politics. As marginalized groups, they typically are low on resources. It is difficult to establish that legal challenges are cost-effective for a social movement. This difficulty is compounded in the case of the Pay Equity movement, since it has had mixed success in using the courts to argue its claims. Early favorable precedents such as *County of Washington* v. *Gunther* (452 U.S. 161 [1982]) were more than counterbalanced by unfavorable decisions, such as *Spaulding* v. *University of Washington*, (740 F.2d 686 [1984]). Yet McCann builds a persuasive case. His argument, in brief, is that the law supplied both a discourse that redefined the problem of low wages for women and minorities in a way that could be won and that the legal costs flowed significantly in two directions. Particularly in the public sector, the price tags associated with litigation and the political costs of appearing to oppose the redress of historic bias against women and minorities were often sufficient for employers voluntarily to engage in job evaluation and reclassification procedures. These resulted in major wage gains for women workers and significant political empowerment.

McCann urges the integration of legal tactics into the social movement literature on resource mobilization and political opportunity. Many of the studies done to date emphasize the limitations of judicial action, its inherent conservatism, costliness, and ability to divert movements from direct action tactics that may be more successful. McCann counters that this is too narrow a view of such tactics. First, he suggests that legal action can and should be instrumental. Its objectives are often far broader than winning one or even a series of cases. It may instead be intended to give definition to a group's discontents, raise the saliency of an issue, and provide a vehicle for collective effort. Second, he notes that resources available for legal action are often not very fungible. Pro bono work by activist attorneys, tax-deductible contributions for legal challenges, and union legal staffs will not likely be available for direct action or protest demonstrations. Legal challenge is intended to be one of many tactics, not the only one or even the lead one. A consequence of McCann's book is likely to be a more careful examination by social movement scholars of the role of legal action. Court cases are often seen as a counterpoint to the serious business of collective protest and lobbying legislative bodies and political executives. Instead, McCann persuades that in America, at least, the courts are a necessary part of societywide efforts at significant change.

As part of this argument, *Rights at Work* challenges the arguments of leftist feminists that liberal rights-based politics is too easily coopted, leaving the fundamental structures of power unaffected. McCann, while acknowledging the limitations of the wage increases won, uses the magnitude of economic gains and the words of labor union women to offer a different interpretation. Within the context of feminist politics, it is difficult to assert that the empowerment and heightened activism of those participating in this struggle will not have lasting ramifications within the political system.

While giving credit to Michael McCann for what he has done in this fine book, there are several lacunae that deserve mention. Although, throughout the book, McCann mentions the importance of coalitions among pay equity activists and labor union leaders, feminists, and civil rights groups, the nature and extent of this cooperation remains vague. Examining the local battles over pay equity, I wondered particularly how much of this grassroots activism was sustained by links to such a broad array of national movements as Laura Woliver suggests can happen in her thought-provoking study of local protest, *From Outrage to Action* (1993). Similarly because the Pay Equity movement seems to flow so directly from the push for equality symbolized for many decades by efforts to add an Equal Rights Amendment to the U.S. Constitution, it would have been valuable to sketch the relationship of the Pay Equity movement and the larger women's movement.

Michael McCann's *Rights at Work* is an insightful historic and theoretical account of the Pay Equity movement. It contributes significantly to our understanding of the intersection of social movements and political institutions, while providing an appropriately expansive view of the role of law in American politics. McCann's conclusion that "such reform efforts can create the potential at least to sustain the momentum for change, and at best to snowball into ever more comprehensive demands and conflicts" (p. 308) is provocative and deserves further testing with other movements.

University of Colorado, Boulder ANNE N. COSTAIN

Privatopia: Homeowner Associations and the Rise of Residential Private Government. By Evan McKenzie. New Haven: Yale University Press, 1994. 237p. $30.00.

By 1992, an estimated 32 million Americans lived in *common interest developments* (CIDs). This represents more than 11% of the population, and the number is growing. Common interest developments include condominiums, coops, and planned unit developments of single-family homes. Why, one might ask, is the trend in housing of interest to political scientists? Evan McKenzie's answer is that this form of housing is governed by homeowner associations, a growing and politically significant phenomenon in urban America. In 1964, there were fewer than 500 such associations; but with backing from the Federal Housing Administration and its private collaborator, the Urban Land Institute, the number had grown to 150,000 by 1992 and is predicted to reach 225,000 by the year 2000.

Homeowner associations can be thought of as a form of privatization of local authority. They exercise controls over residents in matters ranging from the visitation rights of grandchildren to the speed limit on development streets. Covenants, enforced by the officials of the association, often extend to matters of great detail in architecture and landscaping. Association membership and dues are mandatory, much as citizenship and taxes are; and CIDs duplicate many of the services provide by local governments. With a boost from the continuing growth of CIDs, the number of private security guards in the United States now exceeds the number of public police officers.

Common interest developments are a major contributor to what Robert Reich has called the "secession of the successful," a device whereby those with substantial market means place themselves in a private enclave in the company of others of similar tastes and wealth. Having paid for goods and services privately, "secessionists" then resent and resist paying taxes for public services. The services they have paid for, of course, are for members only and therefore not available to the general public. In this way, the idea of an inclusive community or a diverse public loses ground to an overriding commitment to protect property values.

McKenzie views CIDs as a bundle of unintended consequences, a change being promoted without awareness of its social and political impact. Even local governments have facilitated CIDs, looking only at the short-term gains in property tax base. Officials fail to appreciate that CIDs represent "a near-total privatization of the process by which urban and suburban land is converted to residential property" (p. 28). This form of privatization is no mere parallel to contracting out trash collection to a private company. It reshapes the practice of citizenship and is the abdication of a basic power of modern government, "the land planning function itself" (p. 182).

Homeowner associations also exercise considerable control over aspects of the daily lives of residents; but the exercise of power by associations does not give rise to the kind of public debate and participation that might grow out of the exercise of public authority over a diverse citizenry. Because all homeowners share an interest in the protection of the common investment they have made, the incentive to participate and air differences of viewpoint is weak, and associations have been found to foster "a culture of nonparticipation" (p. 25).

Even without a lively internal political life, homeowner associations have emerged as a significant force in state and local politics. They are especially aggressive in pursuing the issue of what they call "double taxation." The associations enjoy the advantages of being organized and of possessing ready channels for political mobilization. Association office holding has become a visible path of recruitment for public office in some states, but the most potent political actors are the professional organizations composed of the property managers employed by the associations. Instead of homeowners speaking as citizens involved in a full set of community responsibilities, they delegate representation to a body of professional managers whose only shared concern is protecting private investments in exclusive housing. Single-issue politics has gained yet another practitioner.

Overall, what McKenzie describes is a major extension of the trend represented by suburbanization—separation into homogeneous communities in which needs and wants are satisfied by one's power as a consumer. Common interest development "housing reinforces and institutionalizes the separation of suburb and city and extends that separation into the city itself." More than that, it "attenuates the sense of responsibility CID residents might otherwise feel for the city as a whole, leading to increasing demands for special tax breaks" (p. 26). McKenzie sees a hardening of class divisions, and in this he is joined by observers as politically divergent as Charles Murray and Robert Reich.

McKenzie traces the development of CIDs back to the work of English utopian Ebenezer Howard, then examines the use of restrictive covenants, follows CID development through the unsuccessful experience with new towns, and highlights the crucial role that capital-rich corporations played in transforming the production and marketing of housing. Through subsidiaries, giant corporations took advantage of economies of scale to build cluster subdivisions that target different levels of affluence. Having gone big-time into housing, these corporate-derived developers broke off from traditional organizations of homebuilders to form the Council of Housing Producers.

Common interest development housing enabled these developers to emphasize exclusiveness, though sometimes differing grades of affluence were separated only by a brick wall. The subversion of public planning meant that housing became simply a commodity to be marketed for maximum return on investments in land.

Common interest developments thus reveal a great deal about market biases. In the United States, the housing industry is oriented more toward the affluent than toward the whole spectrum of consumers. It makes strategic use of economies of scale (and responds to the need to sell all housing in a development) by producing a limited line for affluent and narrowly delineated segments of the market. Above all, the housing market ignores social consequences, especially long-term ones.

McKenzie observes that the shape of our metropolitan areas has been and continues to be determined by government "ratification of decisions made by real estate developers and driven by considerations of profits" (p. 104). These decisions, McKenzie reminds us, fashion far more than the physical landscape; they also shape the character of citizenship in the United States. For those genuinely concerned with the moral fiber of modern America, they might well heed McKenzie's warning about uncritically accepting the privatization of citizenship in an income-segregated society. Market choices have profound social consequences that need to come under public scrutiny.

University of Maryland CLARENCE N. STONE

Between the Lines: Interpreting Welfare Rights. By R. Shep Melnick. Washington: Brookings, 1994. 344p. $36.95 cloth, $16.95 paper.

This is a very straightforward book about a very complicated—not to say crooked—topic. *Between the Lines* is tidily organized into introduction, three case studies, and conclusion. Within each part, the chapters, sections, and even sentences are equally linear and forthright. I emphasize this style because it contrasts so sharply and illuminatingly with the substance of Melnick's concern—the intricate and changeable relationships among the passage of legislation, its codification and implementation, judges' statutory interpretation of that legislation in response to challenges to the implemented law, and passage of new legislation in response to the statutory interpretation. The book bristles with the words *disjointed, complex, fragmented, welter of conflicting claims,* and so on; and it is stuffed full of different actors, agencies, perspectives, assumptions, and strategies. Through it all, Melnick moves smoothly and authoritatively, keeping just enough control that the reader does not drown in detail but always allowing the underlying chaos to shine through the surface narration.

The subject of the book is the cause and consequences of judicial shaping of public policies through interpretations of statutes in ways that allow some political claims victories over others—at least for a while. Judges are increasingly called upon to adjudicate essentially political disputes over vague or contradictory legislation, and they increasingly respond. Those decisions inspire further action in the more conventional political arenas, especially Congress and the executive branch of the federal government, which opens the door for more statutory interpretation, and soon. In combination, all of these actions change laws and their impact in ways that are explicable retrospectively but not predictable or controllable in advance.

Melnick develops this argument by carefully examining three cases: Aid to Families with Dependent Children (AFDC), education for handicapped children, and food stamps. While the narrative structure for the three cases is the same, as is the broad set of dynamic forces that shape each policy's development, the particular history of the interaction between statute and interpretation varies across cases.

Melnick begins with the broad set of dynamic forces that shaped the development of social policy from the 1960s through the 1980s. On the legislative side, he identifies the growth of social reform legislation to the point of "policy congestion," the odd combination of vague goals and specific requirements characteristic of much legislation, the federal government's increased imposition of unfunded or partially funded mandates on the states, and Congress's implied or explicit invitation to litigate new statutes. On the judicial side, he points to judges' declining willingness to defer to administrators' interpretations, increasing strictures on states, growing reliance on legislative histories, and heavier reliance on potential beneficiaries' "actual need," rather than on statutory language as the yardstick for interpretation. In combination, these legislative and judicial changes have led to more statutes to interpret and more interpretation of extant statutes, with correspondingly more response by Congress.

Melnick then turns to the particular histories of his three cases. With regard to AFDC, the courts were initially more liberal than Congress and found something approaching a "welfare right" in the statutes on which they ruled. But with the move from the Warren to the Burger and Rehnquist Courts came a move from broad-principled claims that generally expanded benefits and eligibility for AFDC to an "arid discussions of rules ripped from their context" (p. 111). Congress responded in the late 1980s by shifting the terms of dispute from individual beneficiaries and particular regulations to "efforts at comprehensive welfare reform" aimed at family responsibilities. That move was in part a "repudiation" of "the broad, need-only understanding of entitlements embraced by many judges in AFDC cases." Melnick ends his history, appropriately enough, with a mention of the struggles within Health and Human Services over writing regulations for the new Family Support Act.

The Education for All Handicapped Children Act followed almost the opposite trajectory. In this case, lower-court rulings created a right to "free appropriate public education," which was later enacted into law by a Congress "influenced and instructed by these decisions" (p. 135). Congress actually "exaggerated the scope and solidity of these court rulings" to create a much stronger response to beneficiaries' "actual need" than was necessary to satisfy the courts. The Supreme Court, in turn, has responded by trying to restrict the scope of judicial involvement in creating an implementing this new right, although lower courts have not followed its directive very systematically. Litigation on all aspects of handicapped education flourishes, with no end or discipline in sight.

Food stamps follow yet a different path, starting with an expansive law and even more expansive judicial interpretation of that law. After about a decade, the Supreme Court ordered judicial deference to the implementors (in this case, the Department of Agriculture) and Congress moved to shut off further avenues to litigation. Contention over food stamps continues, but not in the courts.

Melnick explains each of these divergent paths through the same basic variables, almost all political. He addresses the power and desires of constituents; "traceability" of various pieces of legislation; bargaining between interest groups and members of Congress; and maneuvering among individual members, among congressional committees, among judges and justices, and among the three branches of government. He spends relatively little time on doctrinal analysis or the substantive merits (or lack thereof) of particular pieces of legislation. This is, indeed, one of the great virtues of this book for political scientists: it is relentlessly and sometimes hilariously political. (My favorite story comes early, in a description of the maneuvering to determine the official legislative history of the 1991 Civil Rights Act. As Melnick tells the story, no one cared a whit about the actual veracity of the legislative history. All they cared about was including or excluding arguments that the courts might later rely on for statutory interpretation.)

Between the Lines is not flawless, of course. For my taste, it is too quick to attribute illegitimate politicking to the courts and too quick to take members of Congress at their word. For example, if lawmakers do not say that their revisions of AFDC, say, were designed to help whites at the expense of people of color, or the rich at the expense of the poor, Melnick makes no effort to find out whether that is what the lawmakers are actually doing, intentionally or not. He is almost always inclined to be skeptical about the creation of new rights or the expansion of old ones, perhaps without sufficient regard to the justifications of the claimants.

Nevertheless, this is an excellent book, very well written, on an increasingly important but mostly ignored topic, with an analytic structure as powerful as it is understated. With the publication of *Between the Lines*, students of judicial behavior and Congress alike can no longer get away with studying only their most visible piece of the social policy elephant.

Princeton University JENNIFER HOCHSCHILD

Policy and Opinion in the Gulf War. By John Mueller. Chicago: University of Chicago Press, 1994. 379p. $19.95 paper.

This book belongs on the book shelves of students of public opinion and policymaking, especially those interested in foreign affairs. Mueller has provided a definitive sourcebook of U.S. public opinion on the war against Iraq. Its 289 tables offer a comprehensive compendium of data to set straight the historical record of opinion toward the war. These data were extraordinarily extensive because of the salience of the war and, equally important, the enormous expansion in recent years of polling and opinion analysis by the mass media and public pollsters. Only elections, up until now, have typically generated such intense attention to public opinion (though health care reform should eventually be added to the group). Thanks to recent technology, helpful colleagues, and the archiving of the Roper Center, Mueller was able to assemble and analyze the data quickly. Like the data on the Vietnam and Korean wars in Mueller's classic, *War, Presidents, and Public Opinion* (1973), the Gulf War data debunk myths and allow readers and researchers to reach their own judgments concerning the dynamics of public opinion during the periods leading up to, during, and immediately after the war. Mueller's compilation of data and his analysis set a high standard for future books of this sort, which clearly ought to be written on such major policy issues.

Mueller takes advantage of his rich data to examine numerous methodological and substantive questions about public opinion toward the war. He makes excellent use of comparative methods: he gets substantial leverage from comparisons of responses to differently worded survey questions (including response categories and context, which even he acknowledges at one point as "tedious stuff" but essential for making sense of the data) and from comparisons of evidence and insights from the Vietnam War, Korea, World War II, and (interestingly) the War of 1812. What the War of 1812, the Vietnam War, and the Iraq War share was that the political parties were split on the war and that as the war soured (or *if* it soured, in the case of Iraq), the opposition party could not expect to benefit in the long run from its position on the war (pp. 108–12). In 1990, the American public understood why American troops were sent to Saudi Arabia as well as, if not better than, the public had understood what the United States entry into the Vietnam War and World War II was all about. In contrast, as "the most important problem facing the country today," the Gulf War was a tremendous issue; but "it never managed to generate as much concern by this measure as had inflation, drugs, unemployment, or the fear of war at various points in earlier years" (p. 28).

Mueller shows how the public reacted reasonably and coherently to war-related events and developments. Public opinion was hardly volatile or erratic, though it never had to respond to massive casualties. The period leading up to the war, between November and mid-January 1991, showed striking opinion stability concerning support for war as troop levels rose to offensive capability. This stability is particularly noteworthy, given the incentive pollsters had to ask new questions to obtain new information and to repeat questions that would pick up changes. The notable opinion changes leading up to the war were a decline in overall business and consumer confidence (showing that such perceptions and expectations are not just driven by the real economy), an increase in the proportion of people who thought the war would be a short one, unlike Vietnam, and, most important, an increase in the number who perceived a war to be *inevitable*, though there is no evidence that the public would have opposed a "properly packaged settlement" or that the public's pessimism about the effectiveness of sanctions had increased further in weeks just before the war began (pp. 23, 34–36). Mueller may understate the case when he concludes that Bush should have been able to move opinion gradually toward war as Roosevelt had in 1941: the public shared Bush's opposition to Iraq's aggression, its potential nuclear threat, and its war crimes in Kuwait; Bush could promise an easy war, unlike Presidents Roosevelt and Johnson; he had United Nation support and Saddam Hussein's continued misbehavior virtually on cue—to say nothing of several hundred thousand offensive troops poised for action (pp. 60–64).

During the war, there was a "rally effect" in support of the war and the president, with a halo effect on consumer confidence and evaluations of Bush and his presidency that were not directly related to the war (e.g., his personal qualities and his performance in domestic areas, though not in the handling of poverty and homelessness). Mueller compares the Gulf War's rally effect with that for other wars, though fully to compare these effects (as Richard Brody and others have shown), it is necessary also to compare the degree of consensus versus dissensus among other elites that was communicated to the public through the mass media. There was only a slight falling off of support for the war as it unfolded, which was reversed with the ground offensive and rapid victory that came with few casualties. The public's one lingering regret was the Saddam Hussein was not removed from power.

Among other aspects of the aftermath of the war, the book tracks the war's immediate consequences and its long-term place in history. The Vietnam experience had a part to play: the "Vietnam syndrome" in the public mind could be pronounced dead, but perhaps only because Bush chose to end the war without what could be called the "quagmire-of-all-battles" to unseat Saddam—though a contributing factor might also have been worldwide (including some American) dissent in seeing U.S. tanks rumble through Baghdad. Despite some dissatisfaction about what the turmoil that Saddam was able to continue in Iraq and the effect of the war on the Iraqi people, the war has thus far continued to be recalled favorably. In contrast, the effect on the public's attitudes toward the unseated George Bush is now history; and the war had no long-term halo effect on the American public's satisfaction with their own country.

The book's concluding section provides an apt and useful retrospective on the war with regard to the impact of polls on policy, wartime casualities and enemy strategy, the Democrats, the role of the media, war and democracy, peace in the Middle East, nuclear weapons, oil, and other issues. The war faded remarkably quickly, much like the last "big game." Mueller may have neglected one last comparison with the War of 1812. While he reminds us of the ascent of Andrew Jackson and William Henry Harrison, who kept alive positive images of that War of 1812, we have yet to see fully what Colin Powell, Norman Schwartzkopf, or others will do.

Columbia University ROBERT Y. SHAPIRO

The Decline of Comity in Congress. By Eric M. Uslaner. Ann Arbor: University of Michigan Press, 1994. 204p. $37.50.

This is an interesting, provocative, rather daring book, written in an easy, straightforward, lively style. I enjoyed reading *The Decline of Comity in Congress* despite its litany of the deterioration of civility in public life. The book is interesting because the author tackles the really puzzling and worrisome problems for our collective life that are precipitated by rancor in deliberation and, more generally, by the erosion of comity in congressional decision making.

It is provocative in arguing that Congress itself is not primarily to blame for the diminution of civility. Rather, Uslaner contends that there exists a strong linkage between congressional incivility and the waning of communitarianism in America. Accordingly, he argues that structural reforms of Congress, while perhaps desireable in their own right, cannot be expected to reverse the decline of comity. This can only be accomplished by changing the values and norms of the larger society that hinge on the realignment cycles of the electoral process. Otherwise, "if we toss the rascals out either through the ballot box or term limits, we will only get a different set of scoundrels."

The book is daring for its application of multivariate regression analysis to time series in levels of charitable contributing and the public's trust in people, religion, science, medicine, and government, sometimes embracing very few data points. When his case counts drop to eight or nine, Uslaner argues that the results remain convincing because the coefficients are "robust" and in the right direction. And he ingeniously constructs the evidence to show that societal values like trust seem to shape congressional norms significantly more than endogenous ideological modulations brought about by congressional membership turnover.

In reaching the conclusion that the decline of congressional comity is largely driven by the ebbing of values and norms in the society at large, Uslaner discards the conventional institutional causes. These include the view that comity abatement has been occurring because of (1) the congressional reforms of the 1970s, (2) divided control of Congress and the presidency, and (3) the effects of membership changes. In addition, Uslaner considers the argument that decline in congressional norms of civility stem from the impact of the mass media, particularly television. Each of these causes is considered, savored momentarily, and then rejected. Structural reform of the institution cannot, he reasons, account for normative disintegration, because the latter has occurred in both reformed (House) and less-reformed (Senate) bodies.

Divided government may contribute "to the problem of incivility by adding a sharper partisan edge to the already widespread waning of congressional norms," but Uslaner argues that split government is not the real culprit. Now, he would add that incivility abides with both divided and undivided government. Comity has not recovered since the 1992 election gave the Democrats control of both ends of the avenue.

Then, Uslaner argues that comity decline cannot be properly attributed to changes in the congressional membership when, in fact, turnover is relatively glacial. Ideological changes flowing from the infusion of new members of Congress are not the cause of growing rancor and incivility. Rather, Uslaner contends, "the behavioral indicators of congressional norms respond to changes in societal fundamental values (and norms) and not to swings in legislators' ideology." Finally, Uslaner concludes that the media are not to blame, that "the images of Congress and of legislators on television are reflections of the larger society."

Ultimately, Uslaner's analysis is discouraging for those who hope that old-fashioned comity can be restored through reform of Congress, or by way of purification of campaigning and elections. Civilized public discourse can only come, says Uslaner, through modifications in more fundamental societal values and norms. In short, "congressional norms reflect societal norms" and "something beyond the confines of Capitol Hill—a sense of communitarianism in the nation—drives congressional behavior." It may be that "people don't want to admit that members of Congress are stymied because there is so much dissensus and rancor in the larger society" and that "tinkering" with congressional rules and organizational design will not work.

As animated and spirited as Uslaner's contentions are about congressional comity, the interpretation is debatable. Surely, it is true that Congress is not as pleasant a place as it was in the 1950s and that today, debate can become nasty. Uslaner admits, of course, to the partisan coloration of much of the rancor. But he, like many students of American legislatures, shrinks from a theory of politics in which partisan polarization, partisan ideology, party organizational stakes, party leadership effectiveness, and party control are centerpieces. And yet, as a regular, addicted reader of House and Senate debate in the *Congressional Record*, I notice rough edges and unfriendly remarks that are, seemingly, precipitated by very real, even ideological, differences between the two parties. These policy differences were cross-cutting in the halcyon Rayburn era. Today, the important domestic and foreign policy issues sharply divide the political parties. I like party conflict; but I know that to many, party conflict seems merely part and parcel of a decline in comity. I think it is manifest that comity has declined but, I am inclined to believe, not as substantially as Uslaner alleges nor with the serious consequences his assertions imply.

The Decline of Comity in Congress is a stimulating book, the achievement of a perceptive and insightful student of the congressional world.

Ohio State University SAMUEL C. PATTERSON

COMPARATIVE POLITICS

Political Dynamics in Contemporary Japan. Edited by Gary Allinson and Yasunori Sone. Ithaca: Cornell University Press, 1993. 311p. $42.50 cloth, $14.95 paper.

This valuable collection of essays present and debate the political changes since 1970 in Japan. Its writers provide explicit examples (particularly about labor and finance) illustrating specific cases of public policies and private interests heretofore overlooked in the literature on political economy in Japan.

Perhaps the key contribution is the sophisticated yet clear approach to debates about the frameworks for study of state and markets or (specifically in Japan) of the "developmental state" or political marketplace. Such debate can distract from research that tackles the complex, vital, and expensive issues of regulation and change so much in need of analysis in Japan. It is a credit to the editors not only that they gathered a set of solid contributions but also that the chapters were well edited and focused on specific themes and case studies.

Political analysis during periods of rapid political change in Japan will sustain its value only through evidence and analysis about stable structures and underlying dynamics. This is the single most useful volume for analyzing new evidence and debating changes in recent Japanese politics from 1970 to 1993.

This 1993 publication went to press before Japan underwent stunning changes in party politics. Most obvious among the current changes is that Japan has had four prime ministers in a single year since the volume went to press. Therefore, the period of change under one-party dominance that the writers seek to explain is now history. The shortcomings of a volume about political change thus come with the territory but do not negate the study of many of the underlying structures and dynamics shown by the authors.

Less obvious a weakness is the theoretical heterodoxy that is unresolved by the editors. While one editor writes an introduction to such differences in a noncontroversial style, the strongest critical essay on statist theory by Margaret McKean lies deep within the book and is not directly linked to the clear challenge that the volume could have posed. McKean points the way to testing corporatist models that would avoid repetitive research projects that continue to attack or support the straw figure of the developmental state.

Rather than dredging up an anachronistic developmental state model made for the 1950s in Japan, this volume does point the way forward for new research. Looking to assess data from after 1970 (in particular, after 1973, when the impact was felt from financial liberalization, floating exchange rates, and international oil cartels), its targets are the underlying political dynamics determined by the authors to specify such change. The evidence—not presented in a single volume elsewhere—is related to public policy, based in society, and developed over lengthy periods. The case studies draw on a range of sources to provide insights about the accumulated wisdom on Japanese labor, finance, and administrative reforms. A central debate that emerges, through sound comparative analysis, is about the degree to which pluralism and corporatism apply to contemporary Japan. One conclusion is that views differ about whether a type of societal corporatism or patterned pluralism best typify political processes in Japan.

With its emphasis on change, this volume might lose readers seeking some discussion of industrial policy and the strong state. The book is also explicit that its concerns are broader than only talking about bureaucrats and politicians under one-party dominance. The essay by McKean remedies this danger to some extent by noting how the "strong state" thesis is soundly criticized by specialists. McKean goes on to discuss the considerations about pluralism and corporatism, with the further conclusion that Japan is building peak associations that are moving its politics toward an increasingly corporatist form of interest mediation. Other authors, most notably Michio Muramatsu in this volume, strongly disagree by arguing that patterned pluralism is a persisting form of mediation throughout the 1980s. In such debates, the reader finds the key concerns of comparative analysts seeking to explain domestic politics in Japan and to analyze the patterns that provide stability amid apparent change in political parties and parliamentary coalitions.

In sum, this work is a sorely needed addition to our studies of contemporary politics and policy processes in Japan and, by comparison, in all industrial societies. Its findings about national sovereignty, deregulation, and variation among policy sectors are useful for students of comparative public policy—particularly, but not exclusively, finance and telecommunications. But all readers seeking a basis for understanding change in Japan will benefit from the themes introduced by this provocative and path-breaking set of essays.

International University of Japan STEPHEN J. ANDERSON

The Political Ecology of the Modern Peasant: Calculation and Community. By Leslie E. Anderson. Baltimore: Johns Hopkins University Press, 1994. 208p. $42.50.

Traditional studies of Latin America all too often assume that "peasants" are the passive victims of capitalism who are structurally incapable of effective strategic and collective action. Indeed, even studies influenced by the notions of a popular "moral economy" and "everyday forms of resistance" tend implicitly to undermine their premise of popular agency by relegating the actions of rural people to the level of the historically trivial. In the study of agrarian Latin America, it is almost always elite interests and global structures that cause anything of real importance.

However, this whole view of Latin American rural politics is becoming untenable. There is too much evidence of the effectiveness of popular agrarian collective action to be discounted as merely marginal or exceptional; and, as the current conflict in Chiapas, Mexico, indicates, there is no reason to suspect that superficially quiescent peasants do not have the capability to act with considerable force and importance. What we need, therefore, is a new theoretical framework that can break decisively with the empirical and conceptual limitations of established interpretations. An attempt to accomplish this ambitious agenda is the greatest merit of Leslie E. Anderson's important monograph, *The Political Ecology of the Modern Peasant*. Anderson's core theoretical starting point is parsimonious and compelling (pp. 1–18). According to Anderson, it is false to assume that peasants lack the strategic resources and cognitive calculus that are necessary for collective action. Indeed, peasants may be more likely than other, more fragmented social groups to be able to overcome collective action problems because of their rationally motivated ties to communities of support ranging from the household and community to supralocal networks of exchange (pp. 8–18). Since rural people are materially constrained to engage in ongoing networks of interdependence with kin and neighbors in order to survive, they can use those networks strategically in often-successful attempts to pressure elites to make substantial concessions to popular agrarian demands (pp. 11–14). The specific forms of collective action may vary widely, depending on such exogenous factors as the openness of state institutions to peaceful interest group politics, but this variation does not result from outside agitation or varying ideological commitments. Instead, forms of action ranging from protest to outright rebellion are mediated through a common repertoire of community institutions and a widely shared consciousness, or "political ecology," that judges the costs and benefits of group behavior in terms of a careful and flexible balancing of individual and community interests (pp. 5–10). As a result, peasant rationality entails both individually self-interested and community-oriented components; and it is this strategic orientation and its underlying institutional interdependencies that must be the focus of any adequate explanation of peasant politics, even in those areas of Latin America where peasants are seemingly marginal actors.

Anderson tests this simple but useful theoretical framework through a combination of intensive anthropologic techniques and a statistical analysis of structured survey data (p. xv). Drawing on research in six villages in Nicaragua and Costa Rica, Anderson shows that community institutions and a strategic calculus of self and group have decisively affected the politics of peasant society in both cases. Although sometimes apparently quiescent, this appearance masks a more fundamental reality of rational collective action that both shapes and adapts to a shifting extralocal context of changing economic and political opportunities (pp. 19–39). The implication of the evidence is equally clear: peasants are not incapacitated by their macro structural position from contributing to the various national and international histories of which they are an essential part. We can only hope that more Latin Americanists and comparativists will build on the work of Anderson and like-minded scholars in order to construct a more theoretically sophisticated and empirically accurate understanding of rural Latin America, one capable of moving beyond the tendency to reduce Latin American peasants to passive vectors of "inevitable" world-historical forces.

There are, however, two related problems with Anderson's work that deserve at least passing consideration. First, Anderson assumes a norm of equality in rural societies that is questionable, especially if such a hypothesis is advanced as a generalization about all or most agrarian social formations. Some research would support a strong counterargument to the effect that hierarchy, rather than equality, is a key, comparatively widespread component of peasant institutions. It is true, as Anderson suggests, that hierarchy and inequality need not always characterize peasant economic structure, but it does not follow from this that other forms of political and kinship inequalities are not characteristic of peasant life.

This observation, in turn, leads to the second and more significant limitation of Anderson's analysis. Simply put, Anderson's assertion that peasants regard inequality as essentially illegitimate is suspect in light of the enormous forms of inequality and hierarchy that can be found in villages and country districts around the world. These are often quite stable societies, and this forces us to question the idea that rural people never accept unequal arrangements as legitimate. Moreover, the very existence of hierarchy in peasant life may provide the leadership and ability to sanction defectors that are requisite to effective collective action. It may be bad to have a boss, but the alternative might turn out to be a classic collective-action problem marked by the inability of anyone to punish free-riders and organize action on behalf of vital interests. As Anderson well knows, the latter outcome is likely to be disastrous for rural people who need to maintain collective solidarity in the face of a frequently hostile world.

Nonetheless, despite these qualifications, Anderson's work is likely to be a major contribution to a new and more sophisticated understanding of rural politics in Latin America. More of this kind of analysis can only be welcomed.

University of California,
San Diego
VICTOR V. MAGAGNA

Welfare Policy and Politics in Japan: Beyond the Developmental State. By Stephen J. Anderson. New York: Paragon House, 1993. 194p. $46.95.

The Japanese Prime Minister and Public Policy. By Kenji Hayao. Pittsburgh: University of Pittsburgh Press, 1993. 349p. $39.95 cloth, $16.95 paper.

National elections in 1993 marked the end of 38 years of continuous Liberal Democratic party (LDP) rule in Japan. Few political experts anticipated the maneuverings, intrigue, and bizarre alliances that followed. Within a year, three prime ministers had fallen from power. Then an unthinkable alliance took form. In the summer of 1994, Maruyama Tomiichi, head of the Japan Social Democratic party, was appointed as prime minister to lead a new coalition government that would include the previously rejected LDP. Japanese citizens found themselves governed by three political parties, two of which had spent the past 40 years despising each other.

The mystery of alliance building among political parties continues to bewilder the Japanese electorate and encourage academic experts to ponder how firm their knowledge of Japanese policymaking might be. Do we have to start all over in crafting our models and generalizations, in light of the forces that are currently transforming the political landscape?

Both of the studies under review were written in the context of conservative-controlled LDP governments. Kenji Hayao expected the LDP to remain in power although he speculated that the position of the prime ministership would "certainly be transformed" (xii) if a socialist was ever appointed. Stephen Anderson's analysis is more historical, and his findings on the character of welfare policies in Japan are less contingent on the conditions of one-party dominance than on a balance of conflict and accommodation involving conservative politicians and bureaucrats in their dealings with distinctive "policy coalitions" (p. 10).

Both studies assert that the country has been served quite well by politics. What also emerges from their research is a central feature of the Japanese policy process—negotiated group particularism in a system of dispersed political power. Based on evidence in these studies, accommodation among competing but never permanently cohesive policy coalitions, anchored in very specific issue areas, is likely to continue as a dominant motif of the Japanese policymaking process.

The literature on the Japanese prime ministership is scanty for good reason. It is conventional wisdom to note that despite some significant constitutional and institutional advantages, Japanese prime ministers are weak and passive, with no policy goals and no agenda to advocate. They hardly figure at all in Anderson's study. But while not a dominant figure in politics, Hayao asserts that nonetheless occupants of the office are capable of "reactive" leadership. Inescapable and frequently irresolvable issues reach the top. As head of government and party president, the LDP prime minister is the only person who can try to coordinate broad policy programs or resolve major conflicts.

Forced to do something, the author asserts that the incumbent can play a major, if not critical, role at crucial times, such as when the Cabinet must reach a decision or when the National Diet is faced by a vote on important legislation. But reactive leadership is important in times like this only when the prime minister can "take an issue behind which there is already quite a bit of support and give it enough of a boost to get it enacted" (p. 192).

The rest of the book deals with three major questions regarding "reactive" leadership in the making of government policy. Why is it that the prime minister has few incentives or resources to help set the agenda in government and little influence over the content of policy? How and under what conditions has he been able to make his impact felt? How is it that the Japanese political system can still "function" without the strong, assertive leadership commonly assumed to be important in the West?

After first placing his discussion of leadership in a comparative context, Hayao considers in some detail the case of a rather untypically ambitious and policy-oriented prime minister, Yasuhiro Nakasone, as he confronted two grand issues of the late 1980s—education reform and tax reform. Why did Nakasone's firm attempt at top-down leadership achieve only mixed success?

The remaining part of the study examines the political, institutional, and other factors that together shape the conditions in which the office holder can have an impact on the policy process: how the prime minister is selected, the dynamics of LDP and opposition politics, subgovernments in policymaking, and the character of the prime minister's staff. In the end, he writes that the prime minister's ability to effect change "depends on whether he applies his limited energy to an issue that has resonance, that is, one that elicits 'sympathetic vibrations' from other actors in the system" (pp. 194–95). On some occasions, the whole system can vibrate as a result of prime-ministerial energy.

Hayao asserts that policy change is most likely to occur in Japan when the prime minister is pulling from above and political groupings are pushing from below. What, then, does politics look like from below, as presented by Anderson? To continue from Hayao, we see highly charged but only loosely connected energy fields, of distinctive and opposing political coalitions, each seeking to dominate parts of the country's welfare programs to their own narrow benefit. Rarely does an energy field include the top.

Anderson's task is to explain why Japan has substantial welfare programs even though the country lacks entrenched vested interests, significant social welfare movements, and a social democratic government led by political parties backed by a unified labour movement. Despite a weak sense of citizen entitlement to welfare rights and without a broad political commitment to the ideals of a comprehensive and universal welfare provision, the Japanese government still provides significant welfare support. Why is this so?

For the author, the key answer is politics. Anderson focuses on what he calls policy coalitions (we might substitute issue areas, subgovernments, or even networks) across different areas of welfare policy: public pensions, social services and health care. Testing a variety of theoretical approaches to social policy, he shows how institutional precedents and the organization of political interests has permitted distinctive and opposing groupings to support a substantial but also fragmented array of welfare programs. Welfare politics is a complex array of competing political coalitions, coalesced around particular policy issue areas; vetoing universal ideals of the welfare state in exchange for specific government guarantees.

Programs in public pensions are marked by divisions among labor unions, farmers, retired people, insurance associations and business groupings. Major conflicts arising within competing policy coalitions over pensions have been resolved by officials in the Pension Bureau of the Ministry of Health and Welfare (MHW) and LDP leaders. Social policies began with initiatives by prewar bureaucrats, developing in the postwar period in a way to favor nonprofessional interest groups. The most important policymakers are specialized policy groupings (known as *zoku giin*) mostly in the LDP and officials in the MHW as they deal with policy coalitions representing welfare commissioners, organized clients, professional welfare workers, local governments, policy experts, and managers of public facilities. Health care policies have involved a different process from the others, marked as they are by a history of government support and considerable nongovernmental provision and by a postwar policy area dominated very often by professional groupings, including especially the Japan Medical Association. At the center of policymaking here will be found officials from MHW, government deliberative councils, policy tribes, and LDP leaders.

Anderson's study is broad in scope, extremely rich in historical detail, and directed not only towards specific issues of welfare politics in Japan but also to larger questions regarding state involvement in social policies. His framework for explanation is not parsimoniously refined, drawing, as it does, rather freely from comparative research, institutional studies, and a substantial body of previous work on Japanese politics. Thus, he argues that conservative, one-party domination can account for only some of the variation under study.

His structure of explanation is complex, but the portrait is not too much of a surprise. Sometimes Anderson's central arguments get lost, his language a bit unclear, his ideas and evidence scattered too far into irrelevant debates. I would have preferred a much more sharply formulated, tightly written presentation.

Hayao's study is the first book-length analysis of the role that Japan's prime minister plays in policymaking. It is suitable for courses at the undergraduate and graduate levels. The appendixes are extremely useful. His empirical findings and general arguments regarding the virtues and even success of "reactive" leadership in Japan should generate controversy. If leadership is constrained, Anderson's much more technical and diffuse study demonstrates sources for change but also the strength of countervailing power and potential for group stasis in Japanese politics. My puzzle for both authors is that if "reactive leadership" and "coalition politics" work so well, why all the stirrings among political parties these days? Why so much disgruntledness among the citizenry about the quality of politics and policymaking?

University of Toronto MICHAEL W. DONNELLY

Conflict and Change in Cuba. Edited by Enrique A. Baloyra and James A. Morris. Albuquerque: University of New Mexico Press, 1993. 347p. $45.00 cloth, $19.95 paper.

Cuba after the Cold War. Edited by Carmelo Mesa-Lago. Pittsburgh: University of Pittsburgh Press, 1993. 383p. $49.95 cloth, $19.95 paper.

Humbled by the failure of fellow scholars to predict the cataclysmic collapse of the communist regimes in Eastern Europe and the Soviet Union, Cubanologists have been straining ever since to foresee Fidel Castro's future. Despite prophecies of imminent doom by journalists, pundits, and exiles in Miami, the Castro regime remains in power, albeit facing the worst crisis in its 35-year history.

These two volumes, both edited by prominent scholars, explore the depths of Cuba's economic and political crisis. Mesa-Lago's tends to focus more on economics than politics—perhaps not surprising, since he is the most prominent economist studying Cuba. He contributes two articles, one examining Castro's antimarket Rectification Policy, adopted in 1986 (most of which has now been superceded by events), and a second, exploring the regime's strategy for coping with the catastrophic economic collapse (a 50% decline since 1989), caused by the rupture of Cuban trade relations with the former socialist world, and its loss of billions in annual Soviet subsidies. Mesa-Lago concludes that only market reforms offer any hope for economic recovery. But Fidel Castro has always had an aversion to market mechanisms, so Mesa-Lago is pessimistic about the prospects for change and predicts continuing economic decline.

Sergio Roca, writing on the same subject in the Baloyra and Morris volume, is equally critical of the Rectification Policy's antimarket bias; but given the severity of the current economic crisis, he concludes that "when sheer survival is on the line," market reforms may not look so unattractive to the revolutionary leadership. He expects to see Cuba evolve toward a "China model" of transition—an expansion of the market economy combined with continued authoritarian control on the political front.

Recent developments suggest that Roca is closer to the mark. In the two years since these articles were written, the Cuban government has relaxed restrictions on self-employment in the service sector, legalized the possession of U.S. dollars, and restored the free market in agricultural goods abolished as antisocialist in the mid-1980s. The government is currently contemplating additional macroeconomic reforms that would further extend the use of market mechanisms. This is not yet a full-fledged policy of transition to capitalism, such as China has embraced; it is still too piecemeal and, most likely, too controversial within the Cuban elite. But neither is it stasis.

Cuba's position in the world is a key factor in its current crisis, and both volumes contain several articles on international relations. In Mesa-Lago's, Cole Blasier reviews the history of Cuban-Soviet relations, especially their gradual deterioration from the time that Mikhail Gorbachev came to power until the Soviet Union's demise severed Cuba's special relationship with its patron. During the Cold War, Blasier argues, the Soviet investment in Cuba paid strategic and political dividends, so that successive Soviet leaders were willing to sustain it. After the Cold War, Cuba was nothing but an expensive irritant, and Soviet economic aid was on the decline even before the USSR collapsed.

In Baloyra and Morris's volume, J. Richard Planas focuses specifically on the ideological conflict between Gorbachev's reform communism and Fidel Castro's orthodoxy. Planas details Fidel Castro's growing hostility to what he regarded (rightly as it turned out) as the Soviet Union's self-destruction. Turning to the other superpower, W. Raymond Duncan traces the history of Cuba's 35-year stand-off with the United States, noting how skillfully Castro has used—and continues to use—Washington's hostility to enlist Cuban nationalism in support of his regime.

The real black-box of contemporary Cuba is the political system—both the intraelite debate over how to deal with the crisis and the attitudes of the mass public. Surprisingly, Mesa-Lago's book has only one article devoted to politics—an essay by Jorge Dominguez that examines the importation of Gorbachev's reformist ideas into Cuba during the late 1980s. It offers a window on the intraelite debate during this period but does not try to go beyond that limited agenda.

The Baloyra and Morris collection has six articles that focus on politics. One by Baloyra offers a conceptual scheme for thinking about the intraelite debate—a six-category typology based on whether the person (or group) thinks that the current politicoeconomic regime must be retained, modified, or radically reformed and whether they think this can be achieved with Fidel Castro or without him. Unfortunately, Cuba's closed system prevents us from knowing where key personalities or institutions stand on these issues, and Baloyra wisely resists the temptation to speculate. The result is an interesting theoretical schema with little empirical content.

Articles on the Cuban Communist party, the armed forces, and the intelligentsia offer good histories of how these components of the political system have evolved since 1959; but the authors have trouble discerning how they are responding in the current moment of crisis.

If it is hard to decipher how the Cuban elite assesses the need for change, it is nearly impossible to judge the views of the average citizen. No one doubts that discontent over the economic depression is widespread: everyday life has become a demoralizing struggle, as the flood of "rafters" fleeing to Florida in the fall of 1994 demonstrated. But to what extent does the population blame the government for the mess, and what are they willing to do about it?

Damian Fernandez' article in the Baloyra and Morris book chronicles the growing alienation of Cuban youth; but few of them exhibit enthusiasm for any brand of politics—loyalist or oppositionist. Juan del Aguila's portrait of the Cuban dissident movement argues that its constituency is growing, but he acknowledges that few people are willing to take the dangerous step of openly declaring their opposition. Aguila suspects broad, inchoate support for the dissidents; but it is a suspicion that cannot be verified so long as the regime's repressive apparatus remains intact.

In a concluding chapter, Mesa-Lago and coauthor Horst Fabian compare Cuba to the defunct communist regimes of Eastern Europe. Beneath the obvious structural similarity of the Leninist political and economic institutions, they find enough differences to make it doubtful that Castro will become a belated domino.

Imposed after World War II by the Soviet Red Army, the Eastern European regimes had little inherent legitimacy. Nationalism in the region had traditionally been directed against Russia, further crippling elites dependent upon Moscow, both politically and militarily. Cuba, by contrast, had an authentic, broadly popular revolution, fueled in part by nationalism directed against U.S. hegemony—hegemony that Castro wasted no time demolishing.

In Eastern Europe, rapid collectivization and repression of the Church created many enemies. In Cuba, collectivization was slower, the small-holding peasantry smaller, and the Church weaker. Many of the enemies created by Cuba's revolution emigrated to the United States.

Eastern Europeans could see an alternative model of society nearby in Western Europe. Cubans comparing their country to the rest of Latin America saw a region that was economically stagnant during the 1980s and still has millions of people living in abject poverty unknown in Cuba since before the revolution. Cubans might look northward to the prosperity of their exiled cousins in Miami, but the prospect that Miami Cubans might return conjured fears (stoked by the government) that they would restore the pre-1959 regime, with its racial, as well as socioeconomic, inequities.

In most of Eastern Europe, significant dissident movements preceded the final crisis of the communist regimes. At the critical moment when mass protest erupted, these dissidents focused and channeled popular discontent into coherent movements for regime change. In Cuba, the dissident groups that arose in the 1980s have faced intensified repression. None has more than a few dozen members, and none has any observable public following.

Finally, in Eastern Europe, the communist political elite was split between those who hoped to follow Gorbachev's example of reform communism and hardliners defending the status quo. Regime paralysis at the moment of crisis was an important determinant of how easily they were deposed. In Cuba, despite some intra-elite debate about how to cope with the current crisis, Fidel Castro remains a minimum winning coalition all by himself. There is no evidence of significant divisions within the Cuban Communist party or the armed forces (not, at least, since the trial and execution of General Arnaldo Ochoa on drug smuggling charges in 1989).

Fidel Castro's regime, Mesa-Lago concludes, is not likely to fall any time soon; but neither is it likely to undertake the needed radical reforms. His "gloomy" prediction is for continued economic decline and growing discontent, ending in violent civil conflict reminiscent of Romania.

Baloyra and Morris also conclude that Castro retains the loyalty of a significant portion of the population and that the regime retains "foundational legitimacy" that makes its imminent demise unlikely. Change in Cuba is inevitable, they conclude; but they cannot identify its mechanism or its modalities. The regime seems united, in full control of its "Stalinist" repressive apparatus, and reigning supreme over an "obliterated" and "subjugated" civil society—a kind of totalitarian model redux.

Most Cubanologists sense that Cuba is on the brink of monumental changes that will profoundly transformation the Castro regime or destroy it. Karl Marx observed that revolutions occur when people can no longer go on living in the old way, and Cuba certainly seems near that point. But will change come from above or below? Will it be swift or gradual? Will it be violent or peaceful? Will it be led by Fidel Castro, or will it consume him? The articles in these two books are like the readings on meteorological instruments: they give us some sense of the prevailing conditions. But in truth—the lessons of Eastern Europe notwithstanding—we can no more predict the details of ruptures in the social fabric than we than can predict which tropical storm will turn into a hurricane or where it will go.

America University WILLIAM M. LEOGRANDE

The West and Eastern Europe: Economic Statecraft and Political Change. By Thomas A. Baylis. Westport: Praeger, 1994. 239p. $55.00 cloth, $18.95 paper.

The collapse of European communism in 1989–91 made faculty libraries, university courses, and an entire subdiscipline in political science obsolete. Western policymakers, slowly freeing themselves of Cold War stereotypes but alarmed by the region's instability, are seeking guidance from East European specialists. The specialists in the 1990s are themselves groping to explain their own countries without any generally meaningful paradigms to make sense of the constantly changing daily events. Relevant frameworks of analysis, real knowledge of the countries, and (quite simply) updated books about postcommunist Eastern Europe—all are in critically short supply.

Search no further than this exceptionally well conceived and written study by Thomas Baylis. The book succeeds on three levels. First and foremost, it is a comprehensive and incisive overview of the Eastern European political economy in the 1990s. Baylis focuses specifically on Bulgaria, Romania, Poland, Hungary, Slovakia, the Czech Republic, and former East Germany. He explains the common and unique problems of the seven from their historical origins in 1945–89 under the domination of the Soviet Union. The themes of economic integration between East and West Europe, East-West trade, Western investment, technology transfer, and money-credit-debt make up four logically integrated chapters on the choices and dilemmas for postcommunist Eastern Europe in the 1990s.

Second, Baylis reviews the differing motives and the cumulative impact of economic policies toward the region by the United States and other Western governments over 50 years of international relations since 1945. For Baylis, Western economic statecraft—divided in its goals between the United States and the Federal Republic of Germany—still, if unintentionally, brought about the demise of communism in Eastern Europe. The indebtedness of the communist regimes to Western banks and lending institutions by 1989 forced the rulers of Poland and Hungary to concede power through competitive elections to their internal opposition, escalating into the collapse of communism both in these countries and throughout Eastern Europe.

Third, Baylis argues for a logical course of coordinated economic policies for Western leaders and private lending institutions in the 1990s to aid the reform process in concert with Eastern European governments. The West must be more willing to open up its markets to cheaper goods from Eastern Europe, to tolerate restrictions on Western imports by a region nurturing its own fledgling private sector, and to forgive the temporary political lapses and economic reversals from reform by Eastern European governments in their difficult transition period. Baylis warns that the West should not direct its economic policies solely to benefit the private sector in the region at the cost of weakening their national governments. Strong Eastern European governments alone can foster the political climate of confidence and stability for economic growth and finance, with Western aid, the reforms of education and the building of roads and communications essential for fundamental change.

Whether the West commits itself to an equivalent Marshall Plan of economic recovery for Eastern Europe or only adopts smarter policies through its governments and private institutions, Baylis contends that the West's interest in the success of these Eastern European countries lies beyond the region itself. A strong Eastern Europe, which, for Baylis, would additionally include the Baltic countries of Estonia, Latvia, and Lithuania, is the solution for the Soviet successor states of Russia and Central Eurasia, beset by even greater internal problems and posing the real threat to the West by their instability. Eastern Europe is the natural market for many of the countries in Russia and Central Eurasia. The ripple effects from a strong Eastern Europe would stimulate trade between the two regions and economic growth in Russia and Central Eurasia through the demand for its industrial and consumer goods, energy resources, agricultural products, and raw materials.

Sponsored by the Twentieth Century Fund, the book is a grand synthesis of the very best recent monographs on Eastern Europe and analyses written over the past few years by economists and others in journals like the *RFE/RL Research Report* and *East European Politics and Societies*. Baylis has drawn his insights from two decades of research on the former East Germany and from interviews of officials throughout the region in 1990–92. The book would be an ideal choice for a wide range of university courses in international relations and international political economy. Indeed, I was impressed enough to bring the book to the attention of my colleagues in international relations at Iowa State and to urge their considering it for their own related courses.

In its scope, balance, and clarity, the book is the 1990s equivalent of such three-decades-old books as Zbigniew Brzezinski's *Soviet Bloc* and *Alternative to Partition*. Brzezinski's books defined the realities of communist Eastern Europe for a generation of students and influenced Western policymakers in the 1960s and 1970s. Baylis has written the overview of postcommunist Eastern Europe for us to teach and for the West to base its policies on in the 1990s.

Iowa State University JOEL C. MOSES

Utopia Unarmed: The Latin American Left after the Cold War. By Jorge B. Castañeda. New York: Knopf, 1993. 498p. $27.50.

The Latin American Left: From the Fall of Allende to Perestroika. Edited by Barry Carr and Steven Ellner. Boulder: Westview, 1993. 256p. $52.50 cloth, $17.95 paper.

Whether or not the end of the Cold War and the emergence of a new order implied "the end of history" as Francis Fukuayama's much-cited article suggested ("The End of History?"*National Interest*, Summer 1989), those developments seemed to many analysts to mark the end of the trail for the political Left. With the implosion of the supposed "evil empire," the Right had lost its cover story; but the Left had lost something even more important—its dream.

The two excellent books under review here are very different in approach, organization, and coverage; but they raise the same set of questions and arrive at more or less the same conclusions. The questions are, In light of the monopolistic grip of neoliberal paradigms and policies on Latin America, what is left of the Left, and where is it going? Or in Castañeda's words, the quest is "to ascertain whether the relevance of the Left in Latin America is intact" (p. 5).

If one focuses narrowly on the individuals, institutions, and parties that, since World War II, have been viewed as constituting the Left, the answer seems simple enough: the Left is alive and well and living in the Center. However, if one sees the Left in more general terms as the voice of the nonaffluent and if one assesses its viability or relevance in the mid 1990s in terms of the needs or demands of its potential constituency and the prospects of addressing those needs, the subject matter takes on much greater significance.

Both of these books place the experience of the Left in richly woven context, assessing its limited but sometimes heroic victories along with its more frequent and often tragic failures. And both address the learning experience that has brought the Left to its contemporary truce with the overwhelming power arraigned against it. But the volume edited by Barry Carr and Steve Ellner concentrates on the contemporary status of, and challenges to, leftist parties around the hemisphere, whereas Castañeda's bulkier and more ambitious work deals in greater depth with both past and future and with the worsening social crisis that makes the role of the Left ever more trying and crucial.

Ellner's introduction to the edited text highlights three characteristics that distinguish the stance of the Left in the 1990s from that of earlier periods: its commitment to deepening democracy, its acceptance of diversity within its ranks, and its attainment of legitimacy (or merger with the mainstream). Chapters on individual countries (Mexico, El Salvador, Venezuela, Colombia, Peru, Bolivia, Chile, Argentina, and Brazil) suggest that with the notable exception of Peru's Sendero Luminoso (Shining Path), most groups that over the past decade or so operated as guerrillas are now coming in from the cold. They also show that leftist parties, both traditional and new, are holding their own despite the weakness of organized labor by building new constituencies, particularly among nongovernmental, grassroots organizations.

Ellner further notes that the enhanced legitimacy that the Left now enjoys reflects, to some degree, an erosion of support for reformist parties. That would suggest that we have come full circle since the 1960s, when would-be reformists who failed to follow through lost their credibility and their political bases and those who tried seriously to follow through lost their jobs—or even their lives—to military counterrevolution. Whereas some parties on the Left rejected liberal, electoral democracy in principle but accepted it in practice, parties of the Right did just the opposite. They embraced electoral democracy in principle but rejected it when the outcome was not to their liking.

The democracy of the 1960s and early 1970s was unstable precisely because there was hope—and its counterpart, fear—that political democracy might lead to economic democracy. The democracy of the 1990s is more nearly stable because, with the denationalization of economic policymaking, such hopes and fears have been greatly diminished. But the cost of this kind of political stability has been social dissolution—a broken social contract.

One need not see the latest figures from the UN Economic Commission on Latin America to know that the region's income gap is widening dramatically, at a pace accelerated by the growth spurts of the early 1990s. On all sides in the private sector, one sees a new generation of palaces and pyramids.

In Punta del Este and Rio de Janeiro's Barra de Tijuca, in Santiago's *barrio alto* and La Paz's *barrio abajo*, there is a flourishing of gated and guarded communities, ghost towns of mansions used only for holidays and parties. But the *favelas* and *poblaciones populares* and *villas miserias* are growing, too. While all classes are vulnerable to the violent street-level manifestation of law-of-the-jungle economics, the poor are the front-line victims, and in shanty-towns, too, those who can are buying guard dogs.

Meanwhile, the public sector has been asset-stripped. Public servants have seen their standards of living dropping steadily for at least two decades. Services that might pay for themselves or even, at higher cost to consumers, bring in a profit are privatized, while local, state or national governments with no visible means of support are left to pick up the rest. For this, they resort generally to regressive taxes, like Chile's 18% value-added tax. Any attempt seriously to tax the rich would run the risk of setting off a stampede of fleeing capital.

With options so narrowed, pressures so fierce, and the vulnerability of elected leaders so great, it is hardly surprising that the entire political spectrum in Latin America has shifted sharply to the Right, leaving the Left, in programmatic terms, about where a cautious Center used to be. Some would interpret such shifts as surrender, or at least battle fatigue. These books do not. They view this moderated stance, in general, as pragmatism—the outcome of learning through painful experience to see politics as the art of the possible, to better understand which circumstances and processes offer promise and which actors and sectors are potential coalition partners or tactical allies, rather than implacable foes.

It is not in spite of the huge problems and the pressures that private interests are able to bring to bear against the public interest but *because* of them that Castañeda's recommendations for political strategies and policy alternatives are measured and modest. *Utopia Unarmed* will not have much appeal to readers who identify with the romantic Left or to self-designated moderates who would like to blame the Left for the region's politically tumultuous past and socially unraveling present. But to those who genuinely seek understanding rather than reassurance or redemption and those who have the patience to be instrumental in pursuit of objectives rather than merely expressive, this book might well become a Bible.

Castañeda's answer to the limitations of democracy in Latin America is Jeffersonian, that is, more democracy. He finds nationalism still to be an ideal indispensable to coalition building in pursuit of progressive social change, of growth with equity. But the concept must be reformulated so as to promote transnational coalition building and to accommodate regional economic integration among Latin American states. (I would suggest also that nationalism understood as "a sense of place" might be essential to environmental preservation. The only reliable guardians of any ecosystem are those who do not have the option of leaving.) The amalgamation of European-style social welfare state and Japanese-style public-private collaboration Castañeda advocates may fall short of the socialist dream that has been lost, but it has the merit of real-world referents.

Books that do not quite live up to their titles are all too common. This one, on the contrary, encompasses far more than its title suggests. With a remarkable combination of subtlety and clarity and with reams of supporting data, Castañeda explains how Latin America got into the mess it is in and how it might get out. It is surely one of the most important books on Latin America to come along in the last couple of decades.

Monterey Institute of International Studies JAN KNIPPERS BLACK

The Soviet Social Contract and Why It Failed: Welfare Policy and Workers' Politics from Brezhnev to Yeltsin. By Linda J. Cook. Cambridge: Harvard University Press, 1994. 272p. $45.00.

Linda Cook argues that over the four decades following Joseph Stalin's death, a tacit agreement between the communist party leadership and the Soviet working class shaped social policy and working-class politics. In this purported contract, the regime guaranteed secure employment to all workers, low prices for essential goods, social services such as medical care, and wage equality. In exchange, the workers consented to the Party's monopoly and complied with authoritarian political norms. For at least a decade, this thesis has been called the "social contract" by Western Sovietologists.

In developing her social contract thesis, Cook offers a descriptive survey of welfare policies and labor politics under Leonid Brezhnev and Mikhail Gorbachev. She summarizes the criticisms of egalitarian social policy by Gorbachev-era academicians and proposals for reforms that would have undermined employment security, wage equality, price stability, and the provision of social services. Cook's examination of the emergence of labor activism, the transformation of official trade unions, and the emergence of militant independent unions provides a valuable history of events between 1986 and 1991. To "test" her thesis, she carefully crafts a research strategy to examine "pressured decision points" in which Soviet leaders faced a clear choice between maintaining the increasingly costly social contract or abandoning it for reforms.

Cook has undertaken a daunting analytic task. The social contract "denotes an exchange between regime and society in which each party tacitly committed itself to deliver political goods valued by the other" (p. 3). To validate this "hypothesis of a regime-society deal," she must demonstrate that both the Soviet leadership and workers acted as though they were constrained by an agreement and as though they expected the other side to abide by its terms.

Some Sovietologists may not be convinced by Cook's evidence from the Brezhnev era. Cook would have made a more convincing case if she had looked beyond Soviet statistics for wages, prices, and social spending: in the command economy, money was not the most important lever available to the regime for allocating resources. For example, the author's case concerning wage equality in the Brezhnev era needs to be reconciled with the loud complaints about the nomenklatura's privileges that were hidden by these numbers. Her arguments concerning a post-Stalinist social contract are undermined insofar as some of the policies that are purportedly part of this regime-society deal, such as the commitment to full employment (as an obligation as well as a right), dated from Stalin's rule. Similarly, her case is subverted by the evidence that real growth in per capita social spending actually declined steadily from its peak in the 1950s, that is, declined throughout the period of the purported contract (p. 50).

Perhaps equally controversial among specialists will be Cook's treatment of Gorbachev-era policies. She argues that "the Gorbachev leadership implemented policies which led . . . to an abrogation of the contract in the spheres of basic social provision and price stability" (p. 151). Cook seems to treat the criticisms raised by Gorbachev-era academicians as though they were the policies of the Gorbachev regime. This is particularly odd, since many of these academicians became severe critics of Gorbachev precisely because he failed to implement their proposals. Chapter 5 demonstrates that even though Gorbachev announced grand plans (e.g., the comprehensive program for 1986–2000) and introduced some minor reforms, he left most reforms on the drawing boards and continued to raise wages and to expand social benefits in order to buy off the workers. If there was a contract, there is no explanation for why Gorbachev's continuation of the Brezhnev social policy failed to bring worker quiescence.

Area specialists may also debate Cook's attempt to link the absence of labor unrest prior to 1986 and its rise after that year to the constraints of a tacit agreement in which workers gave their consent to the regime. For example, Cook explains that labor unrest became most severe in the coal mines, because this sector never benefited from the social contract to the same extent as other sectors of the economy (p. 168). This begs the question why the miners did not strike under Brezhnev: What had changed since the Brezhnev era? Many specialists may conclude that the explanation is far simpler and more obvious than the social contract: it was democratization. Most striking is the absence of evidence that one would expect to find if workers had really believed that they had been wronged by the regime's abrogation of an agreement, even a tacit one. Nowhere does the author cite evidence that workers complained that the regime had violated a contract. Nor did labor unions appeal to a broken contract to validate their demands. Labor's primary demands constituted an agenda that is familiar around the world: wages, benefits, job security, and greater worker control. Political demands such as removal of Communist party organs from the workplace and removal of Gorbachev belie any agreement by labor to acquiesce in the norms of the regime.

Although it is not clear that it will convince many who do not already believe in the social contract thesis, this very readable book will benefit specialists interested in the Soviet era: senior scholars and graduate students will find a useful review of the case for a social contract that was made by scholars in the 1980s. This book helps us to focus the debate over the utility of this term. For those outside the field, this book may offer data for fruitful comparisons. Although Cook has chosen to cast this study as a monograph in Soviet studies and does not examine parallels with other countries, she presents a wealth of descriptive material that may be of interest to political economists studying the attempts by authoritarian regimes to buy compliance with social programs.

University of California, San Diego PHILIP G. ROEDER

Industrial Relations and European State Traditions. By Colin Crouch. New York: Clarendon Press, 1993. 407p. $72.00.

Contemporary students of comparative politics may be excused for assuming that they are alone in grappling with the complexities of the comparative method. Their Americanist colleagues are firmly protected by "exceptionalism"; their International Relations colleagues have the obvious excuse that there is only "one world" and, hence, only one set of system properties to study at a given moment in time. Nor are other social scientists likely to be of much help these days. In the zealous pursuit of universalistic science, economists have stopped teaching "comparative economic systems" and psychologists rarely discuss the implications of "national character." Most historians and many anthropologists take refuge in thick layers of particularistic description about singular persons, occurrences or societies.

There is, however, one body of work—alas, rarely read or cited by American political scientists—that has long been exploiting the comparative method with subtlety and success. I am referring to the literature on industrial relations. Colin Crouch's *Industrial Relations and European State Traditions* is the culmination of a distinguished career in pursuit of an understanding of why the organized interests of capital and labor interact, conflict and bargain with each other in such diverse ways and with such diverse results across European societies. Contrary to much of the early theorizing by Selig Perleman, Arthur Ross, Clark Kerr, Walter Galenson, F. H. Harbison and John Dunlop, Crouch argues convincingly—and documents painstakingly—the failure of European industrial relations practices to converge toward the American model of pluralism, "business unionism" and decentralized collective bargaining.

In this magisterial work, he draws from the rich (if admittedly Euro-centric) corpus on industrial relations, first, to develop a comprehensive and formal theory of the logics of exchange that lie behind different institutional configurations. Of special significance is the way that he goes about distinguishing between sub-types of pluralist and corporatist systems. For example, his contrast between "hierarchic" and "articulated" forms of the latter takes one well beyond the initial, more primitive distinction between "state" and "societal" corporatism—and, in so doing, opens up for inspection the crucial issue of how interest organizations relate to their members in systems that might otherwise look quite similar. Then, in chapter 3, he situates these abstract-rational models in specifics of political space and historical reality, stressing concrete factors that influence both the "horizontal" scope of bargaining and the "vertical" level of aggregation at which it takes place. More than any other work that I am familiar with, Crouch manages to combine the deductive and inductive approaches, even if (I am pleased to report) it is the subtlety and insightfulness of the latter that ultimately predominates.

The middle section of the volume consists of an exhaustive analysis of more than "a century of institutional development" (1870–1990), in some 15 Western European countries. Through successive "snapshots" at ten to fifteen year intervals, Crouch provides us with historical data on the changing general pattern of industrial relations and on the specific characteristics of the emerging trade union movements. In its detail and comprehensiveness—but not in its design or explanatory structure—the study reminds me of Ruth and David Collier's prize-winning analysis of critical junctures, regime dynamics and the labor movement in Latin America, *Shaping the Political Arena* (1991).

In addition to the usual suspects—density of memberships, powers of affiliates, types of organization, differences in internal authority structure and nature of shop-floor representation—Crouch introduces some rather unusual ones, such as the proportion of organized labor that finds itself in sectors exposed to international competition. In a first, he even includes systematic, if often incomplete, data on the emerging structure of employer associations and their collective behavior. Not only are all this data generated and juxtaposed to various performance indicators, but Crouch concludes each time segment with an intelligent assessment of the extent to which the findings at this point verify or falsify competing hypotheses concerning the causes of different systems of European industrial relations. I cannot imagine a single scholar with a substantive interest in this topic who would not profit from reading and carefully "digesting" these analyses, as well as the very original empirical material upon which they are based.

And that is not all! Having come to the conclusion that existing rival theories do not explain all of the diversity in outcomes, Crouch concludes with three chapters that open up further exploration into (1) "the religious base", especially its influence on the state-forming process; (2) "the historical legacy," especially the role played by pre-industrial, guild-like organizations; and (3) "the Social Democratic contribution," especially its independent impact upon the strength and centralization of the trade union movement. Although it figures prominently in the title, "European state tradition" is never convincingly defined or operationalized. Presumably, it emerges uniquely in every case from the interaction of the above three historical factors.

It is impossible for me to conclude this unabashedly favorable review without introducing a note of irony. As was the case with another recent book—Jukka Pekkarinen, Matti Pohla and Bob Rowthorn (eds.), *Social Corporatism: A Superior Economic System?* (1992)—Crouch comes to the conclusion that "neo-corporatism therefore appears from the study as a desirable form of organizing relations among employers, unions and governments—at least for policy-makers committed to the rights of employees to form autonomous representative organizations" (p. 341). Why, then, if its logic of intermediation is so much more likely to lead to better economic and social performance (and, even, according to earlier analyses, greater governability), is "bargained, articulated neo-corporatism" so difficult to sustain? How come in those countries such as Sweden, Denmark, Norway, the Netherlands, Belgium and even Austria that have long been practicing it in one form or another it is currently under such attack? And what about the difficulties in more recent practitioners such as Finland, Spain and Portugal—not to mention those who earlier tried it and failed such as Great Britain and Italy? If neo-corporatism is such a good thing, why haven't more politicians and people learned this? And why are its neo-liberal opponents enjoying such intellectual hegemony?

Stanford University PHILIPPE C. SCHMITTER

The State and the Poor: Public Policy and Political Development in India and the United States. By John Echeverri-Gent. Berkeley: University of California Press, 1993. 312p. $45.00.

This book shows the value of comparative political analysis that is more than analysis of politics within a major world area. It compares in detail three important programs to alleviate rural poverty, one in the United States and two in India. Since the book deals more with the politics and processes of implementation than with public policy or political development, it might have been better titled, *The State and the Rural Poor: Comparative Implementation and Evaluation of Public Policy in India and the United States*. My suggestion is more commendation than criticism, since so little has been written comparatively and analytically, with solid empirical foundations, on implementation, while much has been written generally on public policy and on political development.

The analytical structure of the book is reminiscent of Graham Allison's classic study, *Essence of Decision* (1971). Combining organization theory and political economy, three alternative models are used to illuminate the three case studies: *rational process, conflictual process*, and *organizational environment*.

The third model is found to explain behavior and outcomes best, though each produces useful insights, generalizable across contrasting political systems. None of the approaches is sufficient by itself. The three together produce an understanding of the convergence and interaction of purpose, power, and process.

In the American case—the Farm Security Administration (FSA), a New Deal rural antipoverty agency created in 1937—we see the limits of political support from the top. Even with strong backing from Franklin D. Roosevelt, the FSA (dubbed "rational success, political failure" by Echeverri-Gent) soon ran into heavy political opposition from Southern politicians and the American Farm Bureau Federation, which represented the interests of larger farmers, who were adversely affected by uplifting farm laborers and sharecroppers. Within the administration, the Department of Agriculture, aligned with these interests, obstructed the FSA, which was closed down in 1946. The New Deal pursued conflicting goals to get America out of the Depression, resurrecting a productive agriculture sector and helping its poorest constituents. When these goals came into conflict, the latter lost. Such an outcome is not unique to the United States, of course.

The Employment Guarantee Scheme (EGS) in Maharashtra State of India shows, among other things, the importance of entitlements. Anyone unemployed in the state is entitled to a certain number of days of work each year at minimum wage (about 154 million person-days during the 1980s). While politicians seek patronage advantages from the EGS and bureaucrats manipulate the projects to some extent, they must somehow deliver job opportunities. This emboldens the poor and enables them to organize and make demands for sufficient and accessible employment opportunities.

While the EGS disadvantages richer farmers by keeping rural wages from being driven below the legal minimum, they benefit from the infrastructure created by public works. Women can gain an independent source of income from the EGS. In areas with EGS schemes, the average agricultural wages of men and women laborers are about equal; where there are no schemes, women's wages are about 19% lower than men's (p. 123).

While there are many criticisms of the EGS—that there is much corruption, that the assets created benefit mostly the rich, that the ruling party manipulates the EGS for its political advantage—it has empowered the poor and alleviated poverty more than in most other states of India. That Maharashtra's economy has been growing fairly fast makes it easier to finance the program and maintain political support, but the EGS may itself have contributed something to the rapid rate of growth.

The National Rural Employment Programme (NREP) in the Indian state of West Bengal, which is operated through local governments (*panchayats*) rather than the bureaucracy, has also has benefitted the poor, though less impressively than in Maharashtra. This is partly because fewer resources have been invested in the NREP. Administrators' and technicians' work has been facilitated by local participation provided they make appropriate "adjustments" in their style of operation. *Panchayat* involvement buffers the program from intrusions by politicians, reduces the number of disruptions, and provides detailed knowledge—advantages often predicted for a participatory approach.

While there is persistent corruption, it is not evidently more than occurs through bureaucratic channels, which are less transparent. Corrupt local officials have been ousted both by the governing party and by voters. Gains for the poor have been achieved through the NREP despite continuing elite domination in rural communities. The electoral process is bringing forward a younger, better-educated cadre of leaders, with few links to traditional landowning elites (83% of the fathers of elected *panchayat* heads were cultivators, 65% with less than one hectare).

The three models used in analyzing these cases represent different "logics" of implementation. The rational process view is concerned with ends-means relations, but it underestimates the clashes and distortions arising from politics. The conflictual process view emphasizes relationships of power and interest; but it takes power as given, not analyzing its sources and changes. The organizational environment view, which focuses on state-society relations, deals explicitly with issues and effects of power but not with the internal dynamics of state agencies.

These perspectives, taken together, offer an approach to comparative political analysis that can have broad applicability. Echeverri-Gent's concern with power relations, in the name of political economy and associated with resource dependence, is fruitful. Unfortunately, he treats "political resources" as the source of power in purely nominal, never analytical, terms.

To his credit, Echeverri-Gent has not given up on "rationality" in a time when purposefulness seems to have lost favor. His analysis contextualizes purposes by considering how they are affected by power, regarding neither interests nor resources as autonomous. These are seen as "embedded" in social processes and institutions as well as channeled by culture.

Echeverri-Gent addresses the importance of monitoring and feedback through administrative, political, and participatory channels to modify the ends and means of policy. Implementation, he says, is "rational" not in the sense that any absolute level of rationality is attained but rather because "it is characterized by a process of dynamic rationality in which authorities progressively fashion implementation so that it corresponds more closely to policy objectives" (p. 18).

His strategy for poverty alleviation starts with policy measures to enhance the incentives for political mobilization of poor (p. 205), both for the poor themselves and for political entrepreneurs. He endorses "capacity building" among the poor, supporting A. K. Sen's arguments for investing in education, health, and other services as antipoverty measures. By augmenting the political resources of subordinate groups, he expects to see "a more equitable trajectory of development." What is not addressed is the consequences, for the poor, of upward social mobility. Opportunities opened up for the more capable and energetic among them will likely leave behind a majority of the poor not much better able to advance their interests than before. The need and role for effective leadership from below is an important factor that Echeverri-Gent's otherwise comprehensive analysis unfortunately neglects.

Cornell University NORMAN UPHOFF

The Anti-Politics Machine: "Development," Depoliticization, and Bureaucratic Power in Lesotho. By James Ferguson. Minneapolis: University of Minnesota Press, 1994. 320p. $16.95 paper.

With his volume *The Anti-politics Machine*, James Ferguson has given the critique of conventional development theory a new and imaginative dimension. Drawing on Foucault's notion that ideas are not just "innocent" abstractions but do have real social effects, Ferguson challenges also the structuralist critique of development associated with dependency and related neo-Marxist theories. Development institutions generate their own discourse, thereby creating a structure of knowledge about the object, on which subsequent interventions are based. It is the narrow boundaries of this discourse and the fact that development analysts and practitioners are caught in this particular *episteme* that explain many of the failures of development, not just the material realities of underdevelopment. Especially pernicious, according to the author, is the tendency to "depoliticize" everything it touches—to whisk away political realities, all the while performing, almost unnoticeably, its own preeminently political act of expanding bureaucratic state power. An analysis suggesting that the causes of poverty in the Third World are structural and political, rather than technical and geographical, has no place in development discourse, because agencies cannot plug themselves into such arguments. Analyses of development problems, therefore, are bound to be one-dimensional and slanted in a naive technocratic direction.

The empirical locus of Ferguson's analysis is the small kingdom of Lesotho in southern Africa, a country that was once exporting grains to its powerful neighbor, South Africa, but that due largely to extensive male migration to the South African mines, eventually lost its position as a regional granary. The portrait of the country caught in a regional economic and political dynamic that has changed its fortunes in the past hundred years or so is totally lost in the 1975 World Bank report titled *Lesotho: A Development Challenge*. Ferguson uses this document to show how Lesotho is instead being presented in development discourse as a country not yet incorporated into the modern world, in need of "development" through agricultural improvement but equipped with the institutional infrastructure for national economic planning and hence responsive to planners' blueprints. He follows up this textual analysis with a case study of the Thaba-Tseka Development Project, an integrated development venture in a remote highland region of the country. This World Bank- and Canadian-funded project made livestock development its central concern, without any sensitivity to local perceptions of cattle. Because of their importance as male-controlled assets in a local society where cash and many other resources are easily contestable in the context of extended families, commoditizing cattle failed, and the project went nowhere. After less than 10 years, it was scrapped altogether. The only durable outcome of the project was increased government control over the political opposition strongholds in the mountains. Thus, even if the project was a failure if compared to intended goals, it left behind a particular legacy that people living in the region cannot easily escape.

Ferguson's book is an original contribution to the literature that tries to understand the development problematic. His argument is a breath of fresh air in an environment where conformity with conceptual "fads" is the norm. He strips the development community of its conceptual attire and leaves it naked for all to see.

Many may recognize themselves in the portrait that he paints but others are likely to take offence. There is certainly reason to argue with the way Ferguson went about his study. Some will ask how typical or representative the material is that he has collected. For example, the World Bank, as an intergovernmental organization on whose board government representatives sit, is much more likely than bilateral aid agencies to display the characteristics identified in the book. There is also the question of the temporal context of the 1975 World Bank study on which Ferguson bases his own textual analysis. The arguments contained therein were typical of development discourse in those years, but the latter is not static and has since moved on. The "anti-politics" tendency is much less obvious in the 1990s. In fact, many donors are now specifically asking for political reforms as a prerequisite for further aid. The somewhat stereotypical portrait based on the discourse 20 years ago is not doing justice to the development community, many would argue. To be sure, the old tendencies still persist in some circles (notably in the intergovernmental organizations), but it is hard to maintain that they are mainstream in the same way as they were in the past.

These comments are also indicative of what appears to be a major shortcoming of the book, at least from the perspective of a political scientist—the author's limited attempt to link his own argument to the broader literature on the state and the peasantry in Africa. To be fair, he acknowledges his limited familiarity with this set of arguments, but he also exposes this deficiency by rather summarily dismissing points that would have deserved more careful attention.

This is a paperback edition of a version that was published by another press in 1990. One wishes that the author had taken the trouble to add some additional reflections on the issues raised in the book and the review comments he received upon the publication of the original edition, especially since the book already contains a contemplative epilogue.

These critical observations notwithstanding, *The Anti-politics Machine* is very worthwhile reading. It is a mirror in which all development analysts—past, present and prospective—should view themselves better to understand what they are all about. By using a different way of looking at "development," he has enriched both academic and development discourse.

University of Florida GORAN HYDEN

The Politics of Finance in Developing Countries. Edited by Stephan Haggard, Chung H. Lee, and Sylvia Maxfield. Ithaca: Cornell University Press. 1994. 334p. $42.50 cloth, $16.95 paper.

The Politics of Finance in Developing Countries contains 10 original essays that explore the political determinants of preferential credit allocation and financial liberalization in eight East Asian and Latin American countries. The editors (Haggard and Lee) provide a brief but useful overview of the existing debates on preferential credit allocation in the introduction. In the concluding chapter, Haggard and Maxfield summarize the main findings of the eight case studies in order to present a more general view of why preferential credit programs exist and how these countries cope with external pressures resulting in financial liberalization. In the introduction, the editors lay out what they consider are the most intriguing questions to be addressed: (1) When do governments intervene in financial markets? (2) Does government intervention promote or obstruct economic growth? and

(3) What prompts governments to liberalize financial markets?

They claim that the most popular approach to examine these three questions involves a sectoral analysis whose main contention is that preferential credit programs emerge in response to the political pressures coming from the manufacturing sector. Haggard, Lee, and Maxfield argue, however, that the sectoral approach, although useful in framing the larger question, does injustice to the smaller but critical details that complete any discussion on prefential credit allocation programs. They therefore suggest that the sectoral approach should be supplemented by two other variables. First, according to the editors, the social basis of support for interventionist policies should not be treated as a given, since the interests of manufacturing (or banks) vary across countries depending on the size, orientation, and financial needs of specific firms. Second, the ability of economic agents to extract resources from state agencies differs depending on the ability of the political establishment to ignore business demands and depending on the actual institutional structure of government agencies.

What differentiates this volume, therefore, from others that have examined government intervention in financial markets is its attention to politics, to the activities of state agencies, and to the interaction between interest groups and policy officials. Politics matters, and the case studies are filled with stories that confirm this thesis. The major conclusion from this volume also corroborates the benefit of going beyond a sectoral analysis. Thus, in countries where executives are insulated from political pressures and a central bank free from political tutelage determines monetary policy, credit allocation programs are less likely to emerge. Moves toward financial liberalization—which basically means allowing market developments to influence interest-rate levels—are also influenced by institutional and political arrangements and reflect the relationship between state and society and the autonomy of state officials to pursue their own policy agenda. Therefore, while convergences in interest-rate policies characterize financial decision making in newly industrialized countries in the 1990s, government officials still differ with respect to their commitment to, and enthusiasm for, financial liberalization. Again, an important distinction among these countries is the ability of policymakers to set the agenda and follow through with the necessary measures to meet its goals.

Whether government intervention in financial markets promotes or obstructs economic growth is no simple question. The editors, synthesizing the findings of the individual chapters, conclude tentatively that credit allocation programs made a positive economic contribution when technocrats were able to formulate economic policies without having to worry about popular backlash and interest group influence.

The case studies cover finance politics in eight countries: Korea, Taiwan, Thailand, Philippines, Indonesia, Chile, Brazil, and Mexico. Each chapter is organized around a theme that incorporates variables such as the institutional characteristics of the state (including the power and political interests of the executive) and the insulation of the policymaking apparatus to explain the presence or absence of preferential credit programs and to explore how and why governments are experimenting with financial market liberalization. At the end of each chapter, the authors attempt to summarize what they consider might be regarded as the main advantages or drawbacks of state allocation of credit.

What appears from the chapters is that middle-income countries, with similar levels of development, exhibit quite distinctive patterns of state intervention in financial markets. In Korea, as Byung-sun Choi shows, governments introduced special loan programs for big business to promote growth and to diversify Korea's economic structure by stimulating a conversion to heavy manufacturing. These programs, however, have encouraged the rise of powerful conglomerates which oppose a quick dismantling of interventionist programs and delay the move to liberalized financial markets. Taiwan is often mentioned in the same breath as Korea, yet it followed a different course in terms of financial interventionism. Tun-jen Cheng points out that when the mainland leadership assumed power, it feared the indigenous business community and decided to neutralize its power by withholding preferential credits and by favoring small firms and farmers. Richard Donner and Daniel Unger argue that Thailand, like Taiwan, did not extensively intervene in credit allocation mostly because manufacturing was weak and linked to commercial and agricultural interests. At the same time, an independent and highly regarded central bank retained control over financial market policies and prevented political elites from manipulating interest-rate levels to assist local business. In short, these three Asian countries have had different experiences when it comes to state allocation of credits, although each of them possesses sufficient state capacity to direct economic growth and development.

At the other end of the spectrum are countries where state institutions are captured and plundered by small and wealthy oligarchies. In Indonesia, according to Andrew MacIntyre, state agencies were unable to formulate a coherent vision and policy intervention because the political elite used financial resources to sustain a patronage system. Rice farmers were the main recipients of Indonesia's intervention in financial markets not because the regime wished to promote agricultural expansion but rather because it sought to appease a potentially dangerous social group. Paul Hutschcroft makes the same argument for the Philippines where state agencies are even less able to steer markets than those in Indonesia because of the entrenchment of a small oligarchy that regularly plunders the state coffers for its own enrichment. In the Philippines, state officials or technocratic bureaucrats played no role at all in the formulation and execution of macroeconomic decision making. Subsequently, the Philippines has suffered from a disturbing lack of economic policy coherence.

Latin America offers yet different patterns of finance policies. Laura Hastings shows that Chile after 1973 went the furthest in liberalizing financial markets not because business demanded deregulation but because the military regime was insulated from political pressures and could freely pursue reform policies. The results, at least in Chile, were nothing short of a disaster. Financial liberalization resulted in an appreciation of the real exchange rate and contributed to a severe financial crisis when continued overvaluation became untenable. A massive devaluation led to high debt service costs and subsequent bankruptcies of firms and banks. Because the military regime refused to tighten financial regulations and supervision, banks behaved carelessly and lent too much to too few clients. When exploring why Chile moved so early to open its financial markets, Hastings concludes that it was primarily due to the ideological inclination of the military regime, which passionately believed in laissez-faire policies, while the private sector was too weak to stop the military.

In her discussion of Mexico, Sylvia Maxfield also challenges the utility of a sectoral approach for understanding finance politics. Mexico experienced numerous swings between intervention and deregulation. Preferential credit allocations programs failed to develop because of the presence of an independent central bank that preferred to promote stability over growth and because of a lack of demand from manufacturing. Moreover, in Mexico more so than in other countries, the international environment constituted a much greater force in shaping finance policies than politics, ideology, or business. The last empirical chapter is by Leslie Elliott Armijo who looks at Brazil. She traces how government intervention in financial markets changed over time as a new ruling elite came to power that challenged the prominence accorded to technocratic advisors in policy deliberations. In the first three decades after 1945, Brazilian governments freely intervened in financial markets because of business pressures and because of the military's support for interventionist policy programs. After 1980, democratization undermined the consensus built around technocratic steering of the economy as local politicians and enfranchised voters clamored for more influence over macroeconomic policy and a greater share of the economic pie. Again, a complicated configuration of forces drove state intervention, ending with a dramatic policy shift as the international climate deteriorated and democratization brought a new regime to power.

After reading these chapters, which are packed with historical and empirical detail, one has the feeling that the patterns of intervention are so unique to each country that no amount of theorizing will be able to produce a coherent conceptual framework that answers not only the question when governments intervene in financial markets but also why governments are moving towards financial liberalization. Despite what would appear to be an impossible task, Haggard and Maxfield attempt to do this in the conclusion. Predictably, the initial thesis—political insulation of executive from pressure groups and institutional arrangements of state agencies—undergoes successive rounds of refinement and elaboration in order to account for the variety of experiences of government intervention in financial markets. Ideology, availability of international credit, social and political turmoil, the country's fiscal situation on top of state capacity, business influence, and bank ownership play a role in the formulation of credit policies and determine the politics of finance. Once the editors have incorporated the specifics of each country into their theoretical scheme, not much is left of the original thesis; and most readers will be confused as to how we should weigh the impact of sectoral pressures against such variables as the institutional attributes of state agencies, bank-industry ownership patterns, international conditions, and democratization. There seems to be a genuine tension between the ambitions of the editors to provide a comprehensive conceptual framework to study the political determinants of finance politics and eight chapters filled with empirical and historical data that seem to defy the attempt to find common threads and generalizable propositions.

Nevertheless, this is an impressive project that will be read by many; and it deserves a wide audience, because its theoretical chapters push us to explore the complexities of state intervention in financial markets and because the individual chapters are of uniformly high quality. To a large extent, the basic purpose of the edited volume has been met. The origins of the project arose with the question, How can countries reconcile fast growth with repressed and underdeveloped financial systems? The chapters and the contributions of the editors have made considerable headway in explaining this paradox.

University of Arizona PAULETTE KURZER

Democracy for the Privileged: Crisis and Transition in Venezuela. By Richard S. Hillman. Boulder: Lynne Rienner, 1994. 198p. $36.50.

Venezuelan democracy has fallen on hard times lately. Once this political system seemed a happy exception to the tides of authoritarianism and civil violence that swept Latin America in the past quarter century. With a strong party system, political stability, no deep ethnic divisions, rising equality and social peace, and a military seemingly under control, Venezuela was the envy of its neighbors and the darling of North American analysts. Now all that seems like a dream. The system has been rocked by economic decline, rising civil violence, attempted military coups (two in 1992 alone—the first attempts in almost 30 years), and pervasive institutional decay. These problems have been matched by a tidal wave of criticism from scholars and analysts, who now find the system to constrain competition, twist genuine representation, and worsen inequalities: affirming privilege while offering only the hollow shell of democracy.

This book purports to offer an innovative analysis of the origins and dynamics of Venezuela's current crisis. The author argues that this crisis is rooted in traditions of authoritarianism and privilege, which provide the backdrop for an ingrained culture of protest and endemic violence. He claims that his analysis will be more accurate and insightful than what is now available in the literature because it is "sensitive" to deeper cultural currents and to the impact of historical continuities. These claims are not matched by reality. There is no sustained theoretical argument: the closest we get is reference to the need to study informal processes and institutions. This is supposed to avoid the perils of formalism and ethnocentrism. These are worthy goals but nothing new, and since the author articulates no methodology and seems innocent of references to relevant bodies of research or theory, we are left in the end with little more than a loose collection of anecdotes. But informal does not mean anecdotal, and the author neither collects nor presents systematic data of any kind. He also provides no sustained historical or social argument, no comparative reference, and no effort to situate Venezuelan experience in regional or global (or, indeed) historical perspective.

Hillman sees this crisis as simply the most recent manifestation of long standing traditions of violence which have their roots in continuities of privilege and resulting popular frustrations. The roots of the problem are located in Latin American cultural patterns, whose expression in Venezuela is a hybridization of authoritarianism and pacted democracy. His analysis of the Venezuelan political system is analytically confused, and works with categories that are so gross (p. xiii) as to cripple analysis, were any analysis to be offered. But Hillman confines himself to asserting that "contemporary Venezuela is the product of a turbulent past" (p. 29). Indeed, Hillman believes that this turbulence ("deeply ingrained culture of protest and endemic violence" [p. xiii]) was simply papered over by the democratic system instituted in the late 1950s. That thin veneer has now cracked open. Venezuelans may have constructed representative institutions, but no matter. These structures allow for no "true" representation (undefined) and in any case run counter to the "deeper cultural patterns" that interest the author.

For Hillman, cultures are permanent, ingrained dispositions, and in the particular case of Venezuela, long standing traditions of patron-clientelism, rooted in continuing inequality, that undermine democracy and keep social tensions boiling. These tensions are bound to erupt from time to time, and that is what happened in the country between 1989 and 1993. Stated this way the argument is unexceptionable, but of course one cannot leave matters here. Evidence is required, evidence that links social conditions to political representation, followers to leaders, and conflicting groups to one another. Political outcomes cannot be attributed to cultural inertia *tout court*. These are specific social events in which real human agents respond to concrete conditions. The notion that culture is a human construct, and therefore necessarily changeable, has no place in this view of the world.

Hillman is impressed by the depths of popular anger, visible in university students (not exactly a representative sample of "popular opinion"), in urban structural adjustment protests (as in February 1989) and in the kinds of military coups and rumors of violence that punctuated 1992 and 1993. His account of these events (student violence, urban riots, and the two attempted coups of 1992) is short on hard data and tendentious in the extreme. In his view, all these constitute an "anomic milieu." "Within this anomic milieu Venezuela is a society driven to a large extent by escapism. Consequently the development of civil consciousness has been a casualty of disillusion as well as escapism" (p. 125). It is not easy to know what the author intends here. But what he states is that escapism and anomie (both terms used very loosely) make for ambiguity in a system influenced by both democracy and authoritarianism. The result is that Venezuela is at a critical juncture, on the edge of a transition (p. 126). He goes on later to argue that the transition (from what to what?) is incomplete because "Venezuela has not been able to completely filter [sic] out the traits of the authoritarian legacy" (p. 156). This reviewer has never seen so many disparate terms thrown together in so small a space to so little effect.

Hillman's verdict is that Venezuela is mortally ill with its own history. His prescription is something other than the present. One could of course construct quite a different story from the materials of the Venezuelan crisis. That story might state that despite economic decline, leadership betrayal, political crisis, rising violence and military conspiracy, there were sufficient reserves in "the system" and in the population to defeat two attempted coups, to generate a host of new political movements (including one major new party), to remove one sitting President and institute impeachment proceedings for him and his predecessor, to choose an interim successor, to hold two national elections, and to return a trusted elder statesman to the Presidency—all in the space of a few years. Hard times indeed, but "the system" did not roll over and play dead. Real human agents worked for solutions. They did not always succeed, and they may fail in the long run, but they clearly refused to be passive captives of what Hillman supposes to be their culture. This book is in the worst vein of instant journalism. It presents no new information and adds nothing to theoretical reflection.

University of Michigan DANIEL H. LEVINE

The Comparative Political Economy of the Welfare State. Edited by Thomas Janoski and Alexander M. Hicks. New York: Cambridge University Press, 1994. $54.95 cloth, $17.95 paper.

Variation in vitro is by no means the same thing as variation in natural history, much less in social history. After laying out his famous "methods of agreement and of differences," as well as his often-ignored "methods of residues and of concomitant variation," John Stuart Mill reminded readers that his methods applied exclusively to experimental procedures. Mill then issued a stern warning against their applicability to the tangle of human affairs. Mill identified the chief difficulties in applying his experimental methods to human affairs—not only the complex interaction of causes but also the fact that his methods required a priori a finite and specified set of hypothetical causes. They remained always, fatally vulnerable to the allegation that a hitherto-unsuspected cause was operating.

Subsequent students of human affairs, including Thomas Janoski and Alexander Hicks, have generally ignored Mill's warnings. They have compared complex wholes (most often, states and their subject populations) in hopes of identifying causes of their differences. Their "theories" have commonly taken the form of lists of variables (interacting or independent) supposed to have strong or weak, positive or negative, effects on specified outcomes. They have regarded theories as validated when the variables invoked sort the cases as expected, rather than when they provided full causal accounts of the effects under investigation. The effort has inevitably drawn attention away from causal processes and mechanisms toward correlations across cases and ostensibly critical differences between cases. Logically indefensible except where the investigator can specify and measure all relevant causes of the variation under observation, Mill's experimental methods have proved methodologically seductive.

In the introduction to their valuable manual for international comparison, Janoski and Hicks explicitly invoke Mill, and resolutely aim their teaching at the national level. Individuals, households, places, networks, organizations, events, transactions, and the many other units students of political economy have commonly examined all give way to comparisons among whole countries and analyses of whole-country histories. Under each of four major methodological headings—time series, pooled time series and cross sections, event history, and Boolean analysis—Janoski and Hicks have written or commissioned a general introduction to the method, followed by two or three essays applying it in contrasting ways. Even more so than in King, Keohane, and Verba's recent *Designing Social Inquiry*, method here means quantitative method. Not even rigorous but nonquantitative procedures such as those of linguistics receive attention. The section on Boolean methods, which features categorical variables and a fine introduction to the distinction between variable-oriented and case-oriented analysis by Charles Ragin, provides the chief exception. Like King, Keohane, and Verba, Janoski, Hicks, and their collaborators worry much about the "small-N problem" (as if geophysics, with its n = 1, were an impossible science), offering advice chiefly on ways of increasing the effective number of cases, rather than on the accurate identification of causes.

Within these stringent limits, *The Comparative Political Economy of the Welfare State* offers lucid introductions to crucial quantitative methods and ample illustrations of their uses. Each of the book's sections covers both "external analysis" (comparison among whole countries) and "internal analysis" (comparison, most often over time, within particular countries). Janoski and Hicks have recruited outstanding collaborators: Larry Isaac, Susan Carlson, Mary Mathis, John Freeman, James Alt, Philip O'Connell, David Strang, Chikako Usui, Charles Ragin, and Olli Kangas. Although Strang's essay on decolonization strays outside the boundaries, in general the substantive essays take up pressing issues in the political economy of welfare states: public intervention in labor markets, public versus private investment, social insurance legislation, and so on. Excellent chapter bibliographies round out the book's utility for students and practitioners.

When the work in substantive chapters goes well, what does it produce? In the first instance, it produces estimates of the relative strength of variables among all those the authors consider as possible influences on the phenomenon under investigation. Let us settle for just one example. John Freeman and James Alt seek to explain variations in public and private investment in Britain by quarter from 1955 to 1984, pitting three "perspectives" (each implying a set of strong variables) against each other: (1) too much popular control, (2) too little popular control, and (3) "Technocracy works." Ideally, they say, we should "specify a *general* structural equation model that subsumes all three, in which each equation would express a row or left-hand-side variable in terms of one or more of the column or right-hand-side variables. If we could identify such a model, estimating it would tell us exactly which sets of coefficients actually are statistically significant and are of particular signs and magnitudes, and thus which perspective best accounts for the British experience in the sample time period" (p. 143). Lacking well-identified models to represent the three perspectives, however, they turn to vector autoregression in which the three models feature zero versus nonzero values for different sets of predictor variables. After refined analyses, Freeman and Alt conclude that their findings cast doubt on perspective 1 and give mixed support to perspective 3 but leave perspective 2 in the running. Thus, as usual, the research helps specify what is to be explained but brings us little closer to actual identification of the processes driving public and private investment.

Still, in the complexities of cross-national research, we often benefit from a clearer identification of explananda. Where we have a good causal model in hand, furthermore, we often have no convenient way to test its plausibility and generality except by translating it into inferences concerning the covariation of certain conditions that should reflect the causes in question. In theories of democratization, for example, effective causes often consist of unobservable agreements among powerful actors and mental events among the citizenry at large. In such cases, we can reduce our uncertainty by thinking through how parliamentary alliances, national elections, and social movement demands should vary if one causal account or another is valid. Within the causal shadowland where political analysts so often toil, the Janoski–Hicks manual will at least help practitioners sort out promising explanations from those that make no sense at all.

New School for Social Research CHARLES TILLY

Indebted Development: Strategic Bargaining and Economic Adjustment in the Third World. By Howard P. Lehman. New York: St. Martin's, 1993. 220p. $49.95.

Indebted Third World states face two conflicting sets of demands. On the one hand, they need to satisfy key domestic constituencies in order to maintain political stability and their hold on power. On the other hand, they need to convince their international creditors to continue to provide capital or at least to accept the periodic rescheduling of old debt service. The conflict between these two relationships has intensified as the international creditors have increasingly conditioned their continued support on economic policy reforms that would negatively affect the material interests of those key domestic constituencies. The more governments agree to creditor demands, the greater risk they face of political instability; but if they continue to meet the demands of domestic constituencies, they run the risk of losing the financial support of international creditors.

The book under review seeks to further our understanding of the strategies that governments use to reconcile these conflicting demands. It includes analysis of both sets of relationships: the government's efforts to maintain itself in power during periods of economic crisis and the negotiations between governments and both their public and private creditors. Case studies of Brazil, Kenya, and Zimbabwe are presented to develop the argument further. As Lehman rightly argues, indebted states are involved in a classic *two-level game*, simultaneously negotiating on two different fronts; and both levels have to be incorporated in the analysis for a full understanding of either relationship. Though many scholars have noted this aspect of Third World debt, it has yielded few full-fledged analyses yet—perhaps because it is an ambitious undertaking, cutting across several distinct scholarly literatures and theoretical issues. A burgeoning literature addresses the international dimension alone, while a separate literature focuses on the domestic strategies that governments use to push through unpopular reforms while treating the actions of the creditors as largely exogenous to the analysis.

Lehman's book is praiseworthy for stressing the need to integrate the analysis of both levels. The theoretical framework he develops in the introductory and concluding chapters emphasizes the constraints placed on indebted states by being at the center of the two-level game. It puts inadequate emphasis on the resources and strategic advantages that this also provides states, however. Thus, states are able to blame creditors for their decisions at the domestic level and domestic constituencies for their international-level actions. His analysis thus tends to exaggerate the threats to the stability of indebted governments despite the fact that his three cases have, in fact, demonstrated a great deal of stability through out the period he investigates. Nonetheless, the analysis has the virtue of identifying what is surely the core theoretical issue relating to international debt.

Unfortunately, the book's analysis never fully realizes the author's ambition to integrate the international and the domestic parts of the story. Thus, chapter 2 describes the changing relationship between the international creditors and debtor states with a strong focus on creditor initiatives such as the Baker and Brady plans and barely a word regarding the domestic constraints the debtors brought to the bargaining table. Chapter 3 then shifts gears and analyzes the evolution of economic policy in the three cases: Kenya, Zimbabwe, and Brazil. Here, the donors all but disappear and policy outcomes are linked to a wide variety of domestic factors, from the ideological orientations of state elites to the interests of dominant societal coalitions. Chapter 4 then discusses the debt management strategies of the three states and their relationships with the World Bank and the International Monetary Fund (IMF). This chapter aims to link the two sides of the story but never really does. Although it repeats material from the earlier chapter, it offers little precise information to indicate that interest group pressures constrained, say, the government of Kenya in a specific set of negotiations with the World Bank or that the government of Zimbabwe implemented some policy change to please the IMF despite the pressures of an interest group. The sections on Brazil link, in general terms, the process of democratization during the early 1980s and the increasingly powerful trade union movement to negotiations with the IFIs, but even that stands out as an exception. For the African cases, societal pressures are alluded to but never actually described, and their capacity to influence the government's position in international negotiation is not documented. In fact, unions, professional associations, and other interest groups are exceedingly weak in most African states and usually lack the capacity to lobby decision makers with much effectiveness.

The author is not helped by his choice of cases. They are different in too many ways to allow him to isolate the impact of the variables of interest on the outcomes we observe. It is not only that the Brazilian economy dwarfs those of Zimbabwe and Kenya or that its interest groups are much stronger. The major part of Brazil's debt is private, while the African states' is public; Brazil was a military dictatorship until the mid-1980s then underwent a democratic transition. Kenya and Zimbabwe's regimes, on the other hand, remained stable under one leader; Zimbabwe only became independent in 1980, and its debt problems came much later than Kenya's. And so on. Lehman notes many of these differences in an analysis often rich in insight and nuance, but he seems unaware that they make his central hypotheses essentially untestable with this sample of three. At the end, perhaps as a result, the book's findings are modest—that governments try to minimize opposition to their reform efforts by careful manipulation of the timing, sequencing, and speed of policy changes and that the partial implementation and "slippage" that characterize so many reform programs result from their desire to keep both domestic constituencies and international creditors happy.

Despite these flaws, *Indebted Development* remains a useful addition to the still-growing literature on the politics of Third World debt. Its review of the global politics of debt is fine. It includes interesting analysis of the recent political economy of Zimbabwe, Kenya, and Brazil that is sure to engage scholars interested in those countries. Ultimately, it falls short in its ambitious theoretical claims, but it has sketched out a line of inquiry that will inspire others.

Michigan State University NICOLAS VAN DE WALLE

The German Left: Red, Green, and Beyond. By Andrei S. Markovits and Philip S. Gorski. New York: Oxford University Press, 1994. 393p. $55.00 cloth, $16.95 paper.

The German Left seeks to explain the evolution of the traditional German "red" Left of socialist and communist activists in the 1950s into a "red-green" Left in the 1980s that has crystallized around the Green party. While Markovits and Gorski argue that common societal transformations—the emergence of postindustrial, post-Fordist capitalism—set the stage for red-green parties and movements in many countries, the potentially most interesting aspect of their book is the argument that after Auschwitz and 40 years of German division, there is a particular "Germanness" in the appearance of the German red-green that has eluded much writing on "new politics" in Western Europe (pp. 18–27).

The authors then go on to describe the predecessors of the red-green Left in the 1950s, 1960s, and 1970s in the first four chapters of the book, primarily the antirearmament, antinuclear, and student movements that eventually generated the byproducts of revolutionary splinter parties and clandestine terrorist organizations that tried to wage armed warfare against the West German state. The second part of the book describes in detail the different ideological currents that came together at the founding of the Greens in the early 1980s and deals with the lengthy programmatic statements that Green party conferences have generated before elections. The last part of the book then describes the electoral and organizational development of the Green party and its strategic dilemmas, particularly in the unification process. The authors correctly identify the inability of the Greens to cope with the unification within their worldview as one of the key difficulties the party encountered in the late 1980s.

The German Left is, to my knowledge, the most detailed reconstruction of the prehistory of the Greens and of the strategic developments inside the party in the 1980s and early 1990s published in English. The interpretive judgments of the authors are surefooted and sound, particularly their emphasis on the national dilemma that temporarily paralyzed Green politics. The authors clearly demonstrate an intimate familiarity with the internal life of the German Left. Nevertheless, the book would have benefitted had three themes been developed more fully.

First, the authors set out the theme of the "Germanness" of the Greens well, but they never really come close to exploring the possibilities of this topic. Much of the book, especially the second part on ideology and political program of the German Greens, is not really tied back to this hypothesis, although there may be ample evidence to prove the authors' general thesis. In particular, the strange synthesis between a libertarian, individualist, participatory impetus, on the one side, and a craving for state regulation and collectively enforced conformity for the sake of ecological rectitude and social equality, on the other, may be quite extreme in the German Greens, probably more so than in any other European Green party. This point could have been demonstrated with the aid of the programmatic materials provided by the party.

Second, had the authors pursued this route, however, they would have had to confront a serious methodological problem. The unique "Germanness" of the Greens can be identified only by contrasting it with reds and greens elsewhere. Here applies what Tocqueville already advised students of the French revolution: "You will understand nothing about the French revolution if you study only France!" In this spirit, the Germanness of the Greens would have come into full view only if the authors had compared the party to like-minded political currents elsewhere.

Third, the authors describe the political activities and pronouncements of the German left from the 1950s onward very well; but in order to understand the "Germanness" of this phenomenon, it would have been critical to probe more deeply into the intellectual history of the German Left. The Frankfurt School of Critical Theory and its intellectual surroundings, in particular, are given scant attention in the book, although its longing for liberation or the "completely other" entered a curious mix with its rigid adherence to traditional German elite culture and clearly foreshadows many o the intellectual contradictions and fallacies encountered by the latter-day left Greens. Nevertheless, the book is a rich mine for those who are interested in the details of German politics and a welcome effort to add some liveliness to the often bland descriptions of the German political scene.

Duke University HERBERT KITSCHELT

Washington, Somoza, and the Sandinistas: State and Regime in U.S. Policy Toward Nicaragua, 1969–1981. By Morris H. Morley. New York: Cambridge University Press, 1994. 343p. $69.95.

Capitalists and Revolution in Nicaragua: Opposition and Accommodation, 1979–1993. By Rose J. Spalding. Chapel Hill: University of North Carolina Press, 1994. 314p. $49.95.

These two outstanding books are similar in that both employ a case study approach and are concerned, in one way or another, with Nicaragua. In addition, in their introductory chapters both provide clear conceptual frameworks as well as detailed discussion of other relevant Latin American cases which can be compared with theirs. Finally, both are based on careful research and thorough, varied documentation.

Nevertheless, they also differ in a number of respects. While Spalding focuses on the behavior of the economic elite within Nicaragua immediately before, during, and just after the Sandinista period, Morley is concerned with U.S. policy toward Nicaragua during the Somoza period and for a year or so into the Sandinista government. As such, the locus and nature of the materials researched for these studies were radically different. Spalding spent years collecting primary data and conducting interviews in Nicaragua. Morley, however, based his research on interviews with U.S. "actors" and on English language primary materials—most importantly, previously classified U.S. government documents obtained via the Freedom of Information Act by himself or watchdog organizations such as the National Security Archive.

Ordinarily, I would be skeptical of scholarly case studies of U.S. policy, such as Morley's, which do not show evidence of field research into the reality of the country toward which that policy was made. Too often, academic specialists on U.S. policy accept Washington's basic assumptions about its chosen enemies out of sheer ignorance. Morley, however, is a long term critic of U.S. policy with considerable knowledge of Latin America. He obviously was not influenced in the least by Washington's very distorted "conventional wisdom" on Nicaragua. In so far as he discusses Nicaraguan reality, he does so accurately.

Morley's overall thesis is that U.S. policy in Latin America cannot be understood if one focuses simply on Washington support for, or opposition to, a particular regime type—dictatorship or democracy—at any particular point in time. Rather, he argues that the United States is mainly concerned with the preservation or resurrection of strong state institutions through which Washington can work to enforce its hegemony. Within the state, the key institution from the U.S. perspective is the military. Traditionally, the United States has used training and aid programs to influence Latin American military establishments to ensure that they will act on Washington's behalf to control threats to stability and U.S. interests—even against local regimes. To Washington, a particular regime—be it democratic or dictatorial—will be supported in so far as it protects strong pro-U.S. state institutions.

Morley makes the case for his thesis by discussing U.S. behavior towards Cuba (1952–1958), the Dominican Republic and Brazil (in the early 1960s), Chile (in the early 1970s), El Salvador (1979–1980), and Guatemala (in the first half of the 1980s). He then devotes the body of the book to U.S. policy toward Nicaragua from the late 1960s through 1981. He demonstrates that, as long as the Somoza regime appeared to be in full control of the situation, Washington was quite content to support the dynastic dictatorship. In 1978, however, when the FSLN challenge to the regime seemed to imply a clear threat to the state itself, the United States began calling for an Organization of American States-mediated negotiated settlement—which many dubbed "Somocismo without Somoza"—that would have eased Somoza out while at the same time preserving his "Nicaraguan National Guard"—the core element within the Nicaraguan state. By the time that tactic failed early in 1979, Washington had decided that Somoza would survive. Thus, it renewed support for the regime itself. Early that summer, when it belatedly became aware that Somoza was, indeed, about to be toppled, Washington first tried futilely to convince the OAS to send a peacekeeping force (to block that outcome) and, then, accepting the inevitability of a Sandinista victory, attempted unsuccessfully to pressure the rebel government-in-exile into agreeing to the preposterous idea of preserving the loathed National Guard as part of the new state.

The FSLN victory that July was, for Washington, a worst case scenario come true. The Somoza regime had taken the National Guard and most of the elite-dominated state down with it. The Carter Administration responded to this unwanted outcome with a two-track policy: on the one hand, it extended frigid recognition and aid to the new regime in the unrealistic hope that it could penetrate and regain dominating influence within the new state while, on the other hand, it prepared for confrontation by systematically identifying and providing support—both covert and overt—to those forces which it would later use to confront and undermine the revolutionary regime and state. In that the overall objective throughout was to recreate a U.S. client-state, Morley sees little validity in Robert Pastor's argument (*Condemned to Repetition: The United States and Nicaragua* [1987]) that U.S. Nicaragua policy under Reagan was a marked departure from that of Carter.

Spalding focuses on the behavior of the economic elite in Nicaragua. Rejecting rigidly structuralist arguments about the inevitability of elite opposition to revolutionary reform, she maintains the thesis that behavior of the economic elite in times of accelerated change is shaped by a variety of factors that will differ from one country and one situation to another. Like Morley, she uses much of her first chapter to discuss relevant cases from other Latin American countries. These include Chile under Allende (1970–1973), El Salvador in the late 1970s and early 1980s, Mexico during its revolution (1911–1940), and Peru during the military revolution (1968–1975). She demonstrates that while some elites boldly confronted the process of change (Chile and El Salvador), others were divided or accommodationist in their response (Mexico and Peru). According to Spalding, elite responses under such circumstances are shaped by five major factors: a) the degree to which the elite are unified or fragmented; b) the character of elite organizations; c) the degree to which the elite perceive a threat to their fundamental interests; d) the degree to which the state achieves political institutionalization; and e) the overall success or failure of the state in implementing its alternate economic model. If the elite are unified, effectively organized, perceive serious threats to their vital interests, and face a government with a weak institutional base and grave problems in carrying out its economic program—as in Chile and El Salvador—they will be emboldened to confront rather than accommodate. Where that particular combination does not exist—at all or in part—some aspect of elite accommodation may be seen.

The bulk of Spalding's book is devoted to the Nicaraguan experience. Here we see a very complex picture. Stunted by its evolution under the Somoza regime, the Nicaraguan elite were relatively fragmented and poorly organized. Perception of threat rose (early 1980s) and fell (late 1980s) as the revolutionary government modified its policies. Conversely, the regime's level of political institutionalization and economic effectiveness—at first relatively high—eventually declined sharply. All of this led to a situation in which the elite—though never happy with the revolution—were not always unified in their willingness to oppose it.

Spalding does a masterful job of conveying to the reader the truly remarkable complexity of the interrelationship between elite and state during this period. And she is able to generate some very astute sub-theses such as when she observes that the "complex pattern of confrontation and accommodation that characterized state-capitalist relations . . . contributed to both the deradicalization of the Sandinista revolution in its final stages and a greater continuity of Sandinista reforms in the postrevolutionary setting" (p. 216).

In conclusion, Morley's and Spalding's books are examples of the optimal use of the case study method. The authors give us clearly-expressed theses which are drawn from exhaustive study of their chosen cases but, equally important, are set within the context of other relevant experiences from the wider region—in this instance, Latin America as a whole. As such, these studies should be of considerable interest not just to specialists on Nicaragua, but equally, to the broader fields of U.S. policy and elites and revolution in general.

Ohio University THOMAS W. WALKER

Interests of State: The Politics of Language, Multiculturalism, and Feminism in Canada. By Leslie A. Pal. Montreal: McGill-Queen's University Press, 1993. 330p. $39.95.

Why would a government directly fund advocacy organizations, and continue to do so even when these groups are critical of its policies? When the state financially supports advocacy organizations, what are the consequences for the groups themselves and for state-societal relations more broadly? *Interests of State* addresses these two questions by focusing upon the government of Canada's direct funding for interest groups concerned with official languages, multiculturalism, and women's issues over the period 1969–88. Throughout, Pal's concerns are both empirical and theoretical—namely, to document the Canadian experience and to appraise its implications for weighing the merits of various conceptualizations of the state and its autonomy.

Interests of State succeeds as an empirical documentation of the state's rationale for entering unchartered waters in financially assisting advocacy organizations. A careful probing of government documents, supplemented with interviews with state officials, leads Pal to conclude that "interests of state" motivated direct government funding of advocacy organizations. These "interests of state" included the promotion of a sense of citizenship, national unity, and participation, as well as politicians' goals of reelection. This observed link between official language policies and multiculturalism, on the one hand, and national unity, on the other, is not novel (others have made roughly the same argument); but it gains added credence owing to Pal's careful archival documentation.

Interests of State's bolder and more interesting endeavour comes with Pal's conjectures regarding the ensuing consequences for the Canadian polity of state financial sponsorship of groups committed to women's issues, multiculturalism, and language rights. The consequences, as he sees them, are fundamental and, ironically, at odds with those originally intended. Ottawa's citizenship programs, Pal argues, have given rise to a politics of particularism and fragmentation that has implications for how Canadians mobilize politically, how they frame their political discourse, and (ultimately) how they restrain the state's autonomy. State financial sponsorship of particular subgroups has created a political discourse and politics that emphasizes collective rights to equality of results and public policy outcomes, encourages political claims "based on identity, grievance, and injustice" (p. 259), and promotes in-group solidarity at the expense of the larger national political community. Pal argues that the political mobilization of multicultural, official language, and women's groups that has resulted from state financial sponsorship has pushed the Canadian political spectrum to the left, even as it has rendered the state less autonomous. With women's, official-language, and multicultural groups having appropriated the exclusive right to represent themselves, the state "has, in some measure, [become] as dependent on its groups as the groups have been financially dependent on the state" (p. 264).

Few would disagree with Pal's conclusions that state financial sponsorship has not only enhanced the organizational development and political mobilization of women's, multicultural, and official-language groups but also elevated a politics of identity and status seeking. Nor, with the memory of their pivotal role in Canada's aborted efforts at constitutional reform during the 1980s and early 1990s still fresh, would many analysts deny their sometimes fissiparous and generally complicating effects on Canadian public policymaking. But when one moves beyond the arena of constitutional politics, Pal's conclusions about the autonomy-reducing effects of these particular groups seem overstated. The impact of the latter pales in comparison to the constraints the state faces in an era of international financial markets, corporations engaged in transnational production and marketing, onerous government deficits, and liberalizing trade regimes. The observation that these collective-oriented groups have simultaneously pushed the political spectrum leftward also seems exaggerated, given recent governments' fiscal and trade policy initiatives and in light of voter support for neoconservative political parties. A broader data set than Pal's—one that includes interviews with leaders of women's, multicultural, and language minority groups—would likely have yielded different conclusions about these advocacy organizations' capacities to impose their goals on the state and reduce the latter's independence. In this vein, the feminist movement's own assessment of its impact on the political discourse and policy arena, as portrayed in Jill Vickers, Pauline Rankin, and Christine Appelle's *Politics As If Women Mattered: A Political Analysis of the National Action Committee on the Status of Women* (1993) is a compelling supplement to *Interests of State*.

As a contribution to the theoretical debate on the autonomy of the state, *Interests of State* provides a very useful comparative synopsis of the public choice, neoinstitutional, and neo-Marxist conceptualizations of the state, distinguishing among the state as actor, as target, and as structure. The analytical clarification is unfortunately marred by the author's blurring of the state's *separateness from*, and *independence of*, society. The discussion of the state as actor and as target is convincing; that of the state as an autonomous structure, "generating vectors of consequences" is confusing and suggests a subordinate state, not an autonomous one (p. 56). Pal does recognize that a structuralist perspective must entertain the possibility of the interdependence of state and society but is indeterminate in terms of where the balance of power lies in a relationship of state-societal interdependence. His refusal to be categoric about the state's autonomy is understandable, but lamentable, given the salience of the question in the current era of interdependent domestic and international political economies and societies.

Interests of State is exemplary scholarship. In presenting an empirically rich and substantively provocative account of a politically significant arena of Canadian public policy, it offers important insights into the distinctive features of the Canadian political culture and political dialogue.

University of Toronto GRACE SKOGSTAD

Lesson-Drawing in Public Policy: A Guide to Learning across Time and Space. By Richard Rose. Chatham: Chatham House, 1993. 176p. $14.95 paper.

This is a timely, intelligent, and pioneering book in a new field. It frames questions about learning that expand the intellectual range of public policy curricula. And it offers to increase the practical contribution of professional graduates.

The problems of government are perennial. Across the centuries, it is likely that—in addition to such great questions as human rights and tyranny—the inner councils have continued to debate similar everyday questions: policies for local economic growth, crime rates and moral virtue, the proper education of the young, the burdens of taxation. Indeed, each annual budget cycle or election can be seen as a return engagement with similar issues and an opportunity to draw lessons from experience. Should not governments be learning more rapidly? And cannot we learn more quickly by assessing the wide range of other practices and outcomes in the world?

Rose is concerned with guidelines for drawing better lessons in public policy. He is a distinguished international leader in the study of comparative public policy; his focus is upon lessons from other locations and historical times, rather than only the scientific evaluation of current practices. His book addresses a series of questions: Who searches, when, and how? Searching across time and space; contingencies that affect the drawing of proper lessons; the virtue of time, which turns obstacles into variables. It culminates, intellectually, in seven guidelines that—like a diagnostic checklist for a physician—alert practitioners to the issues they should address in drawing lessons about the applicability of other models.

Because of the pioneering nature of the book, these guidelines are developed as hypotheses. Thus, the book also is an ideal invitation for numerous undergraduate term papers and more advanced contributions to develop this new study of public policy learning.

Within America, there has always been a state-based domestic learning process, of innovation and diffusion, that has defined the story of progress in public policy. But after the Constitution was created (with its explicit attempt to draw lessons from foreign experience), an American continent-sized insularity and sense of superiority and exceptionalism reduced public attention to foreign experiments and inputs. Rose's discussion alerts us to how frequently the phenomenon of international imitation, lesson drawing, and learning actually can be observed elsewhere. (For example, the Japanese, in both the Meiji period and after World War II, sent teams abroad and borrowed—and adapted—selectively and shrewdly.) Today, in Western Europe, public policy specialists may be more knowledgeable about the programs in neighboring European nations than American state officials (or public policy graduates) are about the range of policies and outcomes in different American states.

It is one of the strengths of the book that the author—an American who has built the comparative study of public policy from a European base at the University of Strathclyde, with extensive first-hand experience—illustrates his discussion with numerous examples. It is an enlightening discussion and makes clear that for major portions of the world (especially the former Soviet Union and Eastern Europe), the process of assessing potential lessons from other national cases (and getting the imported adaptations either right or wrong) is at the urgent forefront of professional policy analysis.

Of the seven key guidelines, the author believes three desiderata will not usually flag barriers to potential lessons from other locations in the advanced industrial world. These are the absence of unique elements, the ability to use different types of institutions to implement a program, and the equivalence of resources.

The critical examinations, his reading of history suggests, should take place in four areas: (1) whether there are simple cause-effect links, (2) a relatively small scale of change to the results, (3) an established interdependence with other programs (as in borrowing a package of policies to convert a planned economy to a market economy), and (4) a congruity of values. It is easier—and more likely to succeed—if one imports a Russian-language version of a standard word-processing program like WordPerfect to run on existing Russian-made personal computer clones, rather than seeking to change all Russian institutions.

If any of the four guidelines register a warning, an easy transfer is problematic, and additional thought (at least) must be given to adaptations—as, for example, when the Japanese adapted the perfect competition market economy of the West to shape relations between firms while maintaining more traditional social solidarity within firms.

The *motivation* to search for lessons—and key barriers—are, of course, political variables. Rose recognizes that technocratic improvements are easier than importing lessons that also change status and power relations and the distribution of income. But, he would argue, there *is* a consequential intellectual component to modern public policy. Where there is dissatisfaction, even politically consequential lessons may be adopted, as the fate of communism attests.

And there are important new changes (e.g., the rise of networks and professional identities) in who does the searching and how. (Of the most important sources of ideas reported by recipients of the Council of Governments' Innovation Transfer Program, the top five were informal communications and other professional activities, not traditional political actors.) Indeed, the book includes references to growing data bases that also reflect new, active, professional players in the creation, identification, and diffusion of new ideas: the Council of Governments' Program and the nominees and winners for the Ford Foundation's Innovations Award program administered through the Kennedy School at Harvard.

Are there policy lessons that governments should but do not learn? And is the N large or small? We do not yet know. But Rose cites comparative data to suggest that, as the late Aaron Wildavsky came to believe, ideological door slamming remains a far more powerful barrier to policy learning than earlier generations of policy analysts (and designers of graduate curricula) believed (pp. 102–3).

This observation may point to an extraordinary opportunity. The issue deserves comment because Rose treats ideologies primarily as value differences, rather than also an opportunity for learning (i.e., as testable empirical propositions). Thus, in the 1980s, key truth claims of Reaganomics were never evaluated by American politics specialists; and an extraordinary, decade-long experiment to change the modal personality of the American people to foster economic growth (reduce dependency and increase self-confidence by altering hierarchical relations to government) may have been based upon an hallucination. (That is, there is no scientific evidence, one way or another, that the linkage of individual personality systems in the American polity is sufficiently strong to allow such consequential effects). More lessons, perhaps, can be learned than political systems recognize. And students may be interested to discuss why no science-based American politics textbook yet evaluates the truth claims of such consequential ideas from the political Right.

For teaching purposes (and this book can be highly recommended for classroom use), another interesting discussion question arises from economists' beliefs in universal laws and the total fungibility of their own policy recommendations (pp. 36–38). From specialists in comparative public policy, there are major disagreements about this empirical stance—and thus lessons aplenty that can be learned in the academic world.

Yale University LLOYD S. ETHEREDGE

Westminster's World: Understanding Political Roles. By Donald D. Searing. Cambridge: Harvard University Press, 1994. 498p. $49.95.

Donald Searing has written a splendid book, a book that can be produced by his fellow political scientists as an example to refute many of the allegations against our profession. In an era when we are accused of rushing out works for spurious topicality or to bloat a professional activities report, Searing has produced a long and carefully nurtured work that has taken 20 years to come to fruition. His book is fully informed by major theoretical debates without being in the slightest loaded with jargon. Indeed, the book is delightfully easy to read, partly because it is spiced with unforced references to plays and novels, challenging the widespread belief that all social scientists are philistines. Contrary to the prejudices of journalists about our profession, Searing shows that there are indeed misperceptions and oversights in the conventional picture of how political institutions operate that it takes political scientists to correct. Above all, Searing has written a thoroughly researched book that is a major contribution to understanding both British politics and political institutions more generally.

Although it is common today to evaluate scholarship largely in terms of its "theoretical" implications, it is worth dwelling briefly on the quality of Searing's scholarship. Searing accomplished the astonishing feat (and those who have attempted more modest tasks will appreciate just how astonishing was his feat) of interviewing four-fifths of the membership of the House of Commons at the time of his field work (1972 and 1973.) The interviews generated some thirty thousand pages of transcripts. Yet it is not only the quantity but the quality of the Searing's field work that impresses. Searing has the skill, surprisingly rare in interviewers, of listening and watching carefully in his interviews—of understanding more about his interviewees than perhaps they intended to reveal. We are used to noticing such skills in television interviewers; they are just as desirable in a political scientist.

As noted, Searing's intellectual contributions are to both the study of Britain and to the study of political institutions more generally. Searing's contribution to the study of Britain is clear. He provides the best and most complete picture of how members of the House of Commons in the modern era understand and define their place in their nation's life. Searing documents the existence and relative importance of the roles that both backbenchers and leaders adopt. Backbenchers, for example, fall into four major categories: (1) policy advocates; (2) ministerial aspirants; (3) those who emphasize service to the constituency; and (4) Parliament men, who focus on the institution itself. Leaders fall into four categories (parliamentary private secretaries, whips, junior ministers, and ministers) more (but by no means completely) institutionally defined than backbench roles. This picture contains surprises. For example, the proportion of MPs eager for ministerial office is much lower than is suggested in popular and journalistic accounts. The chapter that describes the work of the whips is the best (because most closely observed) account of their work available. Searing's emphasis on the parliamentary life of ministers is a useful complement to those studies (such as Heady's) that have emphasized their life in Whitehall, in ministries not Parliament. And through the words of Searing's interviewees, the reader is given fascinating vignettes of Britain's varied (and changing) political cultures.

Searing's contribution to the study of political institutions more generally is his attempt to show that the activities of members of Parliament can be understood by an approach that combines attention to the rules and incentives of their institution with careful attention to their individual goals. Members of Parliament are both constrained and enabled by the institution; they are enabled to pursue goals that are not mere products of institutional incentives but are rooted in the thinking, experiences, or circumstances of the individual MP. Members of Parliament, to an important degree, choose which institutionally constrained role they wish to play. Searing's larger purpose in this book, then, is to save the new institutionalism from a tunnel-visioned use of a rational choice perspective, discarding so much of what is interesting about politics in a quest for "theoretical" elegance. Searing succeeds brilliantly in this larger purpose. He thereby provides those who sense the inadequacies of the rational choice forms of the new institutionalism with an example to use against those who pretend that the only alternative to rational choice is incoherent description.

There are, of course, criticisms that can be made of Searing's work. The most obvious criticism (anticipated by Searing in his conclusion) is that the 20 years that have passed since his fieldwork make the study a poor guide to the contemporary House of Commons. Searing has a number of convincing arguments why this should not be so. Yet Searing himself admits that at least the relative importance of the different roles he describes has changed. Most commentators think that more MPs aspire to ministerial office than when he wrote. The general corrosion of self-confidence in the ranks of the British elite over the last 20 years that has made Britain such a self-critical country may well have reduced the proportion of MPs who take pride in their institution. We could assess more confidently how much has changed since Searing's fieldwork if we could derive from his book a more explicit account of what determines the relative importance of the roles that he describes. I suspect that Searing would answer that such an account would involve many factors such as the state of political parties, the political sociology of each constituency, and the character of individual MPs. A more explicit statement of what determines the relative importance of the roles that MPs adopt, no matter how general, would have helped Searing refute critics who argue (wrongly) that his book is a wonderful account of the recent past but not the present. Searing can take pride, however, in a work that should be read not only by those who study British politics but by all who seek to understand political institutions.

University of Wisconsin, Madison GRAHAM K. WILSON

The Irish Constitutional Tradition: Responsible Government and Modern Ireland, 1782–1992. By Alan J. Ward. Washington: Catholic University of America Press, 1994. $19.95 paper.

This is a precise elegant constitutional history of Ireland, north and south. It is an impressive, and even definitive, work. If there is a problem with the book, it lies in the fact that for the southern Republic, at any rate, a parliamentary democratic model of government borrowed largely from Britain has worked in a fairly satisfactory fashion. For many, the interesting questions about Irish politics relate to broader issues such as the relationship between religious and national identity. As Ward admits, when some of Eamon de Valera's personal papers were opened to the public in 1987, this was the theme that obsessed analysts: "Largely overlooked was the question of whether the basic model of government provided in the Irish Constitution ha[d] served the country well." This is Ward's focus, and he provides a lucid account of the evolution of the Irish constitution which includes a particularly interesting analysis of the Home Rule movement, which dominated Irish Nationalist politics from 1874 to 1916. Home rule was the policy of seeking the creation of an autonomous Irish Parliament subordinate to Britain through the maintenance at Westminster of an independent Irish parliamentary party. As an unfulfilled possibility, home rule has provoked much debate. In Britain and Ireland, strong traditions of historical scholarship have presented home rule as having been the obvious basis for a peaceful settlement of Anglo–Irish relations, frustrated by selfish opportunism, physical force and romantic nationalism. Ward is less than sure about this and gives many excellent reasons to support his skepticism. Nevertheless, he is open to criticism on two points. Alan Ward argues all too unproblematically that after home rule, which would have involved some Irish MPs staying at

Westminster, "talented Irish politicians" would have demanded greater autonomy, even separation, for Dublin, because "no Irishman would have been allowed to govern Britain." If so, the leader of the Home Rule party in 1912, John Redmond, did not believe it: "After home rule is granted," he told J. A. Spender in May 1912, "there will be no reason why Irish members elected to Westminster should not join British Cabinets" (quoted by Paul Bew, *Ideology and the Irish Question* [1994], p. 138). Similarly, Ward overestimates the dangers posed by purely economic nationalism for a home rule settlement, because Ireland was increasingly being subsidised by Britain by 1912 and this was well appreciated by Redmondite MPs.

At times, the discussion is perhaps a little too predictable. Irish independence was brought into being by one overwhelmingly strong "national" party, Sinn Fein, which then immediately split. It is repeated that the resultant civil war of the early 1920s "stifled political development"; but without it, it is actually hard to see how a competitive party system could have evolved. Following also the interpretation of another prominent American scholar of Irish affairs, J. Bowyer Bell (*The Irish Troubles*, 1993), it is assumed that in the recent (1992) Northern Irish "talks," John Hume's SDLP was still committed to a power-sharing settlement, as they had been in the 1970s. No serious analysis of the very interesting "talks" papers submitted by that party could support that view. The party feared that such a settlement could not be stable and seemed to favor a very mild form of joint authority (Dublin–London with European Community input) over Northern Ireland. There are examples, perhaps, of some rather dated pieces of "conventional wisdom" in this book. It remains the case, however, that Ward's broader analysis of Northern Ireland is outstanding and in no way reduced in relevance by the recent dramatic and wonderful outbreak of "peace." It should be pressed into the hands of students.

Queen's University of Belfast PAUL BEW

Exposed to Innumerable Delusions: Public Enterprise and State Power in Egypt, India, Mexico and Turkey. By John Waterbury. New York: Cambridge University Press, 1993. 342p. $59.95 cloth, $19.95 paper.

Governments are shedding power throughout the world. In the advanced capitalist countries, reduction of the public sector is the platform of almost every major party. In the developing countries, reform and even elimination of public enterprise is more and more commonplace. For John Waterbury, the *whys* of public enterprise are the puzzle: Why were they so often a feature of the strategy of import-substituting industrialization? Why did they not succeed better in economic terms? Why have they come under political attack? Why is the reform so politically difficult and, on occasion, destabilizing? Waterbury answers these questions with an extraordinary blend of analytical and empirical detail. This book is a model of how to do context-sensitive yet truly comparative comparative politics. Not only does it actually offer comparisons, it offers generalizability and testability. Waterbury does not simply tell us the story of public enterprise in one place and one time. He offers a rich description of four countries with diverse histories and cultures but whose common experiences with public enterprise offer support for his theory-driven analysis and sufficient information to sustain his rejection of most alternative interpretations. John Waterbury raises the level of discussion of important theoretical and policy questions. In the process, he improves the practice of comparative politics.

Waterbury's choice of cases are Egypt, India, Mexico, and Turkey. All have long experience with state-owned enterprises, interventionist states, and relatively large and diversified economies. All have reconsidered—and to some extent reformed—the public sector within the last decade. However, in two (India and Egypt) the reforms are designed to maintain the basic arrangements, and in two (Mexico and Turkey) system transformation, especially through increased privatization, is the aim.

The differences among the four countries make their shared characteristics all the more significant. In explaining the performance and pathologies of their public sectors, it is Waterbury's major contention that the commonality of formal institutions composing the property regime of state-owned enterprise swamp the particularities of class, culture, and history. The property regime creates a series of principal-agent problems that no state (at least no state in his study) seems able to avoid. Its structure of rewards and punishments produces suboptimal outcomes over time. Waterbury assumes rational, maximizing actors, who are caught in a set of institutional arrangements that produce ambiguous hierarchies of command, endless bargaining for advantage, soft budget constraints, and no means to rid the state of failing enterprises. Consequently, the agent, or public-sector manager, lacks strong incentives for high performance and sufficient sanctions for poor performance. The property regime also creates a series of collective action problems that generalize across the countries. On the one hand, the state has created vested interests that block reform. On the other hand, these groups are so internally divided that they can agree only on protection of common entitlements but have minimal capacity to resist major structural changes in the state-owned sector.

Waterbury prefaces the book with a quotation from Adam Smith, a quotation from which he draws his title. The sovereign, it seems, faces an impossible task. It is his duty to promote the general interest, and to do this he must somehow direct private industry. However, the sovereign is human and thus lacks adequate knowledge to foresee the consequences of his interventions. He is thus necessarily plagued by "innumerable delusions." According to Waterbury, the primary delusions of the policymakers who established—and attempt to maintain—state-owned enterprise are (1) their belief that public enterprise managers would be selfless and devoted servants of the public interest and (2) that control of the exploitative private sector and rational design of the public would lead to sustained economic growth. What they fundamentally misunderstood was that even the best designed institutions and the most highly motivated civil servants can change over time, that institutions suited to one time and place may not work for another. The thinking of Douglass North is evident here (and the citations to him are many). Waterbury concludes that state-owned enterprises once contributed to industrialization and development but are now a major source of economic decline.

This brief summary barely does justice to the complexity and richness of Waterbury's argument. It certainly does injustice to the research the book represents. Most works in comparative politics are either single-country studies, syntheses of the secondary material on a range of countries, or highly abstracted statistical or formal analyses. Waterbury is the exception that should be the rule. He packs his text with statistics, interview data, and detailed investigations of the histories, class structure, coalitional structure, and almost every other variable that might be important; and he has done this for four countries.

The empirical and analytical strengths of the book are also its weaknesses, however. The more we learn about these four cases, the more we want to learn about others. Given the amount of work these four cases required, it is unfair to ask him for more; but it is fair to ask him to identify the criteria for further and critical tests of his generalizations.

Furthermore, the mass of detail Waterbury provides obscures, as well as reveals. This points to and reflects a more serious concern. The analysis, particularly the model of principal and agents, is underspecified. Having told us that there are no clear principals and, in fact, a multiplicity of diverse and often conflicting interests in the coalition that comprises the principals and having told us that even the agents are unclear, the application of this perspective is problematic. It is powerful in clarifying the mechanisms that will produce more productive and less opportunistic behavior by agents, as well as those that undermine performance. Waterbury's application goes some distance on the latter and almost none on the former.

The discussion of collective action problems suffers from a somewhat different difficulty. He does not quite succeed in explicating how the same groups are so successful in maintaining entitlements and so unsuccessful in resisting a fundamental overhaul of state-owned enterprises. The missing piece (albeit in his subtitle) is state power. Waterbury offers a wonderful summary and typology of current thinking about the state. He devotes a chapter to the managers of state enterprises and their bureaucracies. What he does not offer is an analytics or even a description of the role of state institutions other than the public enterprises. Legislatures as instruments of constraint receive a brief mention and dismissal. Perhaps the rules that govern policy agendas or the nature of electoral institutions are as epiphenomal as class and culture: they nonetheless demand exploration. The other major lacuna in this book is also political. State-owned enterprises have consequences for political, not just economic, development. The richness of the cases permits an assiduous reader to draw out the implications for political management. It is harder to determine the implications for democratic governance.

Once again, of course, I am asking Waterbury for even more work—in this last instance, on a subject that he has not declared as part of his project. Yet the relationship between state and economic institutions is an underlying assumption he seems to share with most of the other authors in the Cambridge series, in which his book appears. Thus, it is incumbent upon him at least to raise these issues. *Exposed to Innumerable Delusions* is such a good book that I find it impossible not to ask its author to delve deeper and wider. I learned so much from what he did write that I am eager to learn from what more he could write.

University of Washington MARGARET LEVI

INTERNATIONAL RELATIONS

Strategic Nonviolent Conflict: The Dynamics of People Power in the Twentieth Century. By Peter Ackerman and Christopher Kruegler. Westport, CT: Praeger, 1993. 366p. $55.00 cloth, $22.95 paper.

Justice without Violence. Edited by Paul Wehr, Heidi Burgess, and Guy Burgess. Boulder: Lynne Rienner, 1994. 301p. $45.00 cloth, $18.95 paper.

The recent use of nonviolent methods in many large-scale struggles around the world has captured much attention and should draw readers to the books reviewed here. As Ackerman and Kruegler suggest, nonviolent action is likely to be increasingly relevant as nongovernmental actors play larger roles in international affairs, as ethnic conflicts become more widespread, as international intervention in response to human rights abuses grows, and as the technology of communication facilitates and encourages reliance on nonviolent action (pp. xxi–xxiv). The two books complement each other in intriguing ways.

Ackerman and Kruegler's book fittingly has a forward by Gene Sharp and another by Thomas C. Schelling. Believing that good strategic choices in using nonviolent actions will enhance their effectiveness, Ackerman and Kruegler offer 12 principles to guide those choices. They do so, building on the work of Gene Sharp and their own years of analyzing nonviolent action and also drawing from the work of military strategists.

The principles are derived from the analysis of actual nonviolent campaigns, both successful and failed. The principles are formulated to improve the quality of the strategic choices made by activists. Ackerman and Kruegler are not advocates of what is sometimes called *principled nonviolence*, according to which activists adhere to nonviolence in principle, believing it to be morally right and seeking to transform the conflict and the relationship with the adversary. The authors observe that activists have generally been pragmatic and do not adhere to principled nonviolence in that sense.

The 12 strategic principles fall under *development* (e.g., "formulate functional objectives," and "develop organizational strength"); *engagement* (e.g., "attack the opponents' strategy for consolidating control," "maintain nonviolent discipline"); and *conception* (e.g., "sustain continuity between sanctions, mechanisms, and objectives"). The book provides detailed and often gripping accounts of nonviolent campaigns in six cases: the Russian Revolution of 1904–6, the *Ruhrkampf* of 1923, the Indian Independence movement of 1930–31, the resistance in Denmark, 1940–45, the 1944 civic strike in El Salvador, and the Polish Solidarity movement in 1980–81. Each campaign's conformity to the 12 principles is assessed to help account for the campaign's failure or success. The authors consider the campaigns in Denmark and El Salvador to have been successes, the ones in Russia and the Ruhr to have been stalemates, and the ones in India and Poland failures in terms of the immediate objectives of those undertaking the campaigns.

The authors are able to draw many inferences from their analysis. They find that conformity to the principles they propose does help account for the successes. The lessons they draw suggest sensible guidelines for nonviolent protagonists (and their adversaries). The authors present a coherent approach and are clear enough that their inferences can be refuted as well as elaborated and specified further.

The volume edited by Wehr, Burgess, and Burgess is the first major product of the Justice without Violence Project of the Conflict Resolution Consortium, based at the University of Colorado. The contributors are from many disciplines; and, with two exceptions, all are members of the consortium.

The first four chapters provide the theoretical context for the analyses of applications which follow. In chapter 1 Burgess and Burgess review the relevant theoretical literature particularly Kenneth Boulding's concept of power. Boulding suggests that power has three "faces" or sources: *threat*, *exchange*, and *love* (or the integrative system). This conception serves as a theoretical orientation for many of the contributions and is elaborated in chapter 3 by Boulding himself.

Burgess and Burgess discuss the various meanings of justice, observing "that the term means different things to different people, and disparate belief systems have equally valid, but varying views of what constitutes justice, so we declined to impose a common definition in all cases" (p. 258). They simply note that the conflicts examined in the book "are ones most observers would agree are examples of serious distributive or procedural injustice" (p. 9). However, the implications of people disagreeing and agreeing about the meaning of justice and the occurrence of injustice should have been examined further. Moreover, the contributors use the term *justice* in various senses.

Burgess and Burgess, in chapter 2, map out the research questions and hypotheses guiding the project. Doug Bond, in chapter 4, offers a set of definitions and distinctions of nonviolent action as related to social change. He builds on Sharp's work but broadens the concept of power, incorporating the sources noted by Boulding and other analysts. Attention to noncoercive inducements can be of help in linking pragmatic and principled nonviolent action.

Eight quite varied chapters are devoted to nonviolent action in particular countries or regions. Paul Wehr and Sharon Erickson Nepstad analyze the negotiations in Nicaragua settling the conflicts between the Sandinista government and the Indian and Creole peoples of the Atlantic coast and between the government and the Contras. Although occurring in the shadow of violence, the authors focus on the institutional and normative support for nonviolence and how that contributed to attaining justice.

Joel Edelstein surveys the largely nonviolent 1989 revolutions in Eastern Europe and Zaven Arabajan surveys the widespread avoidance of violence in the breakup of the Soviet Union and among the successor countries. Stephen C. Thomas examines the conflict between the Chinese government and citizens seeking political reform, particularly the 1989 democracy movement.

James R. Scarritt uses the Minorities at Risk data set to analyze the relationship between different forms of protest to socioeconomic and political conditions in Africa in the 1980s. He found, for example, that "nonviolent protest is more likely than riot or rebellion to be positively related to at least some aspects of greater democracy" (p. 183). William Kaempfer, Anton D. Lowenberg, H. Naci Mocan, and Lynne Bennett conduct an interest group analysis of external sanctions against the South African government, finding that sanctions seemed to give some immediate encouragement to domestic opposition groups in the target country. Finally, Amin M. Kazak considers belief systems' impact on the conduct of the Israeli Arab/Palestinian conflict and Elizabeth Mathiot (Moen) examines the contributions of development organizations in India to the attainment of justice.

The two books report some similar findings. For example, the authors conclude that those using nonviolent methods are most often pragmatic in their use, rather than principled. They also find that nonviolent action often is accompanied by violence. Even when it is not, each clearly operates with the other as a possible alternative. In a sense, then, they are not so much alternative as complementary methods. That should expand the research agenda and the policy range for those focusing on violence and those focusing on nonviolent action.

There are, of course, significant differences between the books. They have different objectives and formats. The diversity of materials in the Wehr, Burgess, and Burgess book results in a wide array of observations, some of which are not incorporated in Ackerman and Kruegler's 12 principles but may well be relevant for them. For example, in the Wehr, Burgess, and Burgess volume, attention is given to the impact of cultural differences and the content of culture upon the use and effects of nonviolent action. Ackerman and Kruegler tend to emphasize the universality of their principles and are loath to recognize conditions as necessarily limiting the skillful application of nonviolent action.

In the conclusions of the Wehr, Burgess, and Burgess book, the relevance of a protest group's objectives for the likely effectiveness of coercive methods is noted. Certainly, the nature and magnitude of the goals sought affect the likelihood of success of nonviolent action or of any methods. That poses some difficulties in deciding when a group has had success or when enough justice has been attained. Ackerman and Kruegler's focus on campaigns helps deal with the problem but does not resolve it entirely. We need to think much more about when protagonists in a struggle should best move to settle or to escalate the conflict.

Avoiding violence is a widely shared value. But so is the attainment of justice (and of freedom, order, economic well-being, and many other conditions). These works offer some evidence that violence may not be necessary and may even be counterproductive in seeking greater justice. The authors are not sentimentalists about this and give due regard to the evidence about the use of nonviolent action.

These books are important contributions to the study of nonviolent action. They provide challenges to some of the conventional scholarly—as well as popular—thinking about the effectiveness of military and other violent means of coercion. They provide ideas, findings, and theories that warrant further testing and specification. These books are valuable for what they accomplish; their value also will be found in the research, theory, and practice they will undoubtedly stimulate.

Syracuse University LOUIS KRIESBERG

Liberal Leadership: Great Powers and their Challengers in Peace and War. By Mark R. Brawley. Ithaca: Cornell University Press, 1994. 210p. $34.50.

Dissatisfied with several unexplored assumptions in the recent scholarship on the relationship between the rise and decline of major powers, their commercial policies, and the outbreak of wars, Mark R. Brawley sets out to qualify them both theoretically and empirically. His main contention is that the choice between economic policies (free trade, protection, or rent seeking) is defined by the nature of the political system (republic vs. autocracy) on the one hand and the relative abundance of capital and labor in the economy on the other. Their consequences for trade policy differ in capital- and labor-intensive sectors of the economy.

A liberal world economy is primarily a result of the interests by the capital-intensive republic to export capital and processed goods and to create international regimes to undergird such a policy. The accumulation of capital in challenging economies erodes the position of the liberal leader, but it has no way out of its leading role because rent seeking is alien to its political culture and a protectionist shift would hurt the most competitive sector of its economy. If the republic is labor-abundant, it will likely combine a support to, and competition with, the liberal leader. In capital-rich autocratic states, the pursuit of domestic rents becomes the favorite option, while the labor-intensive sectors oscillate between protectionism and free trade.

Brawley's theoretical analysis elevates the type of political system and the intensity and abundance of capital to places of pride. The reasons for this view are obvious; the nature of political governance decides whether the elites can fuse the political and economic control of society to their own advantage, while the strength of capital is an indicator of the material basis for economic and military expansionism. Capital-rich autocracies with strong military power are especially inclined to pursue expansionist policies, but they usually fail to establish large and stable alliances. The offensive strategies of rent-seeking societies are countered by the defensive responses of large coalitions of status quo powers headed by liberal leaders. Brawley rejects, however, the view that major wars are primarily caused by the decline of hegemonic powers and argues that their causes are much more complex.

Liberal Leadership explores the validity of this theoretical model by focusing on five historical periods from the Peace of Westphalen to the present. It deals primarily with the rise and decline of the Dutch, British, and U.S. liberal leaderships in 1648–1713, 1815–1918, and 1945–1990, respectively, but considers also their challengers: France, Russia, Japan, and Germany. Their pursuit for leadership converts the periods of hegemony into commercial and political rivalries for the primacy in international relations. Interestingly enough, in all three periods, the liberal leader has ultimately failed to maintain its prime international position, while its main challenger has not been able to replace it. Challenges mounted by autocratic, rent-seeking societies have revealed, instead, the ultimate weaknesses of their internal economic arrangements and external policies.

Mark R. Brawley's book is an impeccable contribution to the study of international change. His ambition is to develop a parsimonious model of explanation that would help to account for the strategic economic and political choices made by major states and their structural consequences. Parsimony leads inevitably to conceptual and empirical simplifications. While this is true also for *Liberal Leadership,* it is able to retain the coherence and credibility of its main arguments. The author should perhaps have relied more on dyadic information that is now used only in an implicit way. Peter Wallensteen's *Structure and Trade* (1973) suggests that the dyadic (in)compatibility of political systems and factor endowments of major powers would have probably added to both the theoretical sophistication and the empirical richness of the study.

The merits of Brawley's study are, in part, due to the analytical rigor of the book and its concise and disciplined historical analysis, which, as a rule, includes the necessary information and excludes unnecessary details. In this type of study, the author often walks on a tight-rope and threatens to fall either into a trap of inadequate evidence or of theoretical strait-jacket. Brawley has been able to avoid this pitfall quite well. My only complaint is that his information on the accumulation of capital stocks, a key factor in the analysis, is often too vague. The historical material is richer on liberal leaders than on autocratic challengers, but this is justified by the focus of the study.

Obviously, the stress on the process of capital accumulation as a pivot of the rise and decline of nations, regulated by the nature of their political systems, opens Brawley's study to criticism on excessive materialism. He stops short of expanding the analysis to Schumpeterian and Rostowian leading sectors, which would have been consistent with the thrust of his theoretical argument. Criticism on excessive materialism may be justified, for instance, in the case of Brawley's explanation that the capital scarcity of Britain after the Spanish War of Succession (1701–13) prevented it from assuming liberal leadership (pp. 73–74). It looks over a variety of internal constraints in the British society that the republican triumph of 1688 had failed to eliminate. One may also say that the relative size of the British economy was at that time too small to be able to provide adequate resources to lead the world economy. In general, the author should probably have incorporated the size factor more systematically in his theoretical framework as it is conceptually independent of the abundance or scarcity of capital and labor.

A strength of *Liberal Leadership* is that it avoids the trap of a billiard ball model, in which many neorealist studies fall. An effort to understand the rise and decline of major powers without considering the composition of their domestic economic endowment and political constitution cannot but be inadequate. Several recent studies, including a volume edited by Richard Rosecrance and Arthur Stein, *The Domestic Bases of Grand Strategy* (1993), show how important the internal/external nexus is for the understanding of international political processes and their outcomes. Brawley shows in a convincing and elegant manner that internal economic and political arrangements have both direct and indirect consequences for the external policies of major powers. He is able to explain *why* individual states chose between different policy options concerning military expansion and alliance formation, while his theoretical model is less capable of telling *how* such decisions were made. While Brawley takes a long step towards integrating systemic approaches with the *Primat der Innenpolitik,* we still need foreign policy analysis to understand how decision makers convert different constraints and opportunities into policies.

University of Notre Dame RAIMO VÄYRYNEN

Arms and Warfare: Escalation, De-escalation, and Negotiation. By Michael Brzoska and Frederic S. Pearson. Columbia: University of South Carolina Press, 1994. 330p. $39.95.

This is an excellent, well-researched book on the relationship between the arms trade and the conduct of warfare in the Third World over the past three decades. Whereas previous studies have focused on the relationship between arms races and the causes of war, this one considers the impact of arms shipments on a conflict once it has begun—hence filling a niche in a literature wherein arms transfers have heretofore largely been studied as essentially peacetime phenomena somewhat disconnected from the study of war. A central focus is the complex effect that weapons transfers have on the escalation and deescalation of conflicts and on the progress of negotiations and war termination.

The work centers on 10 case studies of recent cross-border wars, providing a mix consisting mostly of high-intensity conventional wars but also including some low-intensity conflicts. Those cases are the India–Pakistan Kashmir War of 1965, the India–Pakistan Bangladesh War of 1971, the Central American Football War of 1969 between El Salvador and Honduras, the South Atlantic (or Falklands) War of 1982 between Great Britain and Argentina, the Yom Kippur–Ramadan War of 1973, the Israeli–Arab War in Lebanon in 1982, the Iraq–Iran War during 1980–88, the Western Sahara War between Morocco and the Polisario guerrillas in 1976–91, the Ogaden War between Ethiopia and Somalia in 1977–78, and the Tanzania–Uganda War in 1978–79. Additionally, the authors have appended a brief analysis of three conflicts that have occurred since the end of the Cold War—the Gulf War, the war within ex-Yugoslavia, and the recent skirmishing over Kashmir by India and Pakistan—so as to provide a picture of the shifts in arms diplomacy during warfare produced by the wholesale changes in the international system and in relations between the major arms-supplying powers. Hence, the authors have provided analyses for all but two or three cases of large-scale conventional warfare over the past 30 years, the exceptions being the 1967 Arab–Israeli War (too brief to allow for arms resupply operations by the superpowers), the intense three-week border war between China and Vietnam in 1979, and perhaps also the lengthy Afghanistan War, which was conducted in a mixed grey area between the conventional and low-intensity levels. They have provided, necessarily, a proportionately smaller sample of low-intensity, cross-border wars.

The wars in their sample comprise a complex mix with regard to the key variables commonly used to compare wars—duration, severity (levels of casualties), and magnitude (sizes of forces involved)—providing a fertile field for analysis with regard to arms transfer diplomacy between the major arms suppliers and their respective clients, the combatants. On one end of a spectrum, there is the titanic, brutal eight-year-long struggle between Iran and Iraq that resulted in at least a million battlefield deaths, in which the resupply of arms to the combatants comprised something like 20% of the total of global arms sales over a period of several years. On the other end, there were the brief skirmishes between El Salvador and Honduras and between Tanzania and Uganda, neither of which involved truly significant intrawar arms transfers.

For each of their cases, the authors provide a four-phased analysis: (1) the background to the war, its causes, and the diplomatic runup to the initiation of hostilities; (2) the prewar balance of power and sources of weapons on both sides; (3) the patterns of weapons acquisitions in the period immediately preceding the war; (4) the course of the war itself and the relationship between diplomacy and intrawar arms flows. Each case study hews closely to this format, and the result is an excellent comparison of the wars, which interweaves the course of the wars themselves, their contextual diplomacies, and the flow of weapons. As a basis for comparison, the authors examine symmetric and asymmetric patterns of supplier behavior as applied to arms resupply and embargoes (which provides a picture of which, if either, side of the war is advantaged or disadvantaged by arms resupply diplomacy), *which* patterns are related to prewar advantages on one side or another with regard to weapons, negotiations during the war over weapons resupply, and finally the wars' outcomes. The authors are then able to hypothesize about the factors leading to deescalation of conflict: arms supply strongly favoring one side (particularly defenders), dependence on single suppliers, high war attrition and painful stalemate, and embargoes and international pressures.

Of particular interest are the analyses in the various cases of the mixed motives of the various big-power arms producer states that must decide whether or not to conduct arms resupply operations on behalf of erstwhile client states. There are commercial motives; but also, a major power may wish to see a certain state win or at least not lose a war so as (1) to bolster its own image of power, credibility, or reliability; (2) to bolster existing relationships with combatants or third parties; (3) to test and prove weapons or doctrines under fire; or (4) to forestall resort to nuclear weapons or outside intervention. By the same token, suppliers may refuse resupply for fear of being drawn into open-ended commitments, depleting their own weapons stocks, exacerbating conflicts with big-power rivals or with worried allies, violating principles of legal or ethical restraint, allowing capture of classified technologies, and so on. There are excellent discussions of these dilemmas, for instance, with respect to U.S. policies toward Israel in 1973 and Pakistan in 1965, Soviet policies toward Iraq after 1980 and Ethiopia during the Horn War, and the crucial nature of U.S. arms assistance to Britain during the Falklands War.

Among the authors' conclusions, to the extent allowed by the small sample size and the diversity of the cases, are that arms suppliers have relatively little leverage over the outcome of hostilities but that the effect of arms supplies on the level of hostility and the occurrence of negotiated settlements varied across cases. Arms deliveries were seen clearly as a factor in decisions to go to war, because of considerations about military superiority and perceptions of changes in the balance of power. Arms deliveries were seen as generally having prolonged and intensified the fighting in many cases. And in general, arms embargoes tended to have stronger effects on short wars and those fought with more sophisticated equipment (Arab–Israel and India–Pakistan), while economic embargoes ultimately had more effect in longer wars (Iran–Iraq) and those among very poor recipients (Tanzania–Uganda, El Salvador–Honduras). Finally, warring parties were found usually able to find new, albeit probably inferior, arms sources in time to keep most wars going despite efforts to stop the fighting by big-power arms suppliers.

Pennsylvania State University ROBERT E. HARKAVY

European Foreign Policy: The EC and Changing Perspectives in Europe. Edited by Walter Carlsnaes and Steve Smith. London: Sage, 1994. 312p. $22.95 paper.

It has by now become a commonplace in the study of international relations that the end of the Cold War should lead to a reexamination of international relations theories. For better or worse, such a reexamination is now in full swing, with the principal focus of debate being theories of international systems. By contrast, theories of foreign policymaking have largely escaped this *perestroika*, a fact that makes the appearance of this book a welcome one; for it is "concerned primarily with foreign policy theory," the aim being "to examine the extent to which the main theories of foreign policy can explain two sets of developments: the end of the Cold War in Europe and the increasing integration of West European states" (p. 1). Which, if any, theories stand up to this test, and how, if at all, should they be modified?

In a symposium volume such as this, one should not expect complete unity of views. Understandably, the authors cover a number of different empirical domains: European states, multinational corporations, and the European Union. Less understandably, though, there is little-to-no agreement on just *what* theories of foreign policy are or should be under examination. The problem here is not, as might be imagined, a surfeit of theories but an almost extreme paucity. One finds only the most cursory references to standard theories of decision making, of bureaucratic politics, of cybernetic processes, of psychodynamics, or of learning. Instead, the various studies tend to operate with a notion of foreign policy making as *zweckrational* activity by individual actors, who, subjected to pressures and opportunities both at home and abroad, pursue goals as best they can. Thus, "the main theories of foreign policy" are, in fact, not examined; and the stated purpose of the book is not achieved.

In and of itself, this failure would not be dire. After all, one might still expect that the individual studies would shed light on particular foreign-policy-related events and processes, even if they have little to say about foreign policymaking in the abstract. To some degree, this hope is borne out, notably in the chapters on Nordic reactions to the Soviet coup attempt, on Swedish candidacy for the European Community, and on Eastern European policies during the final years of the Cold War (by Hans Mouritzen, Bengt Sundelius, and Olav F. Knudsen, respectively). Each of these chapters contains a fairly developed treatment of primary source material and shows quite nicely how government leaders changed their policies over time. Unfortunately, this empirical, richness is not accompanied by an equally satisfactory set of explanations. Instead, one finds ad hoc descriptions: "concern for the Swedish image"; "the policy-makers concluded that their traditional policy was ineffective in dealing with the problem, as presently defined, making the problem worse, generating new problems (i.e. in the domestic election arena) and costing much more than anticipated"; "in a most natural way, however, given the circumstances of Eastern and Central Europe at this time, the statements gave evidence of a process of 'structural probing': agents presented tentative or conditional analyses" (pp. 171, 193, 221).

The other individual studies are rather more difficult to characterize. On the one hand, they contain few theoretical claims about how foreign policy is supposed to be made. On the other hand, their empirical claims are couched at a high-enough order of abstraction as to be of little use for either theory evaluation or theory description. As a result, it is difficult to tell how conclusions in these studies are arrived at or even what they might mean for purposes of research. How, for example, is one to assess the claim that "the process of European integration may be seen as a two-level game" (p. 140) when the author never says what the game is about, who the players are, or what other ways of "seeing" integration are ruled out by adopting the game metaphor? How is one to understand a chapter in which "foreign economic policy theory" is criticized for an "orthodox distinction between what is considered economics and what is politics" (p. 76) when, three pages later, "the European context" is adduced to conclude that "it is plainly nonsense that the theoretical apparatus of foreign policy does not include foreign economic policy"? One is forced to conclude that the volume fails to achieve both the goal its editors set for themselves and the more modest one of contributing in some fashion to our understanding of foreign policy phenomena in Europe.

These shortcomings are too systematic to be attributable to time pressures or the inevitable difficulties of coordinating over a dozen scholars. Rather, they seem to stem from three types of confusion, each of which by itself would be debilitating for serious research. First, most of the contributors to the volume conflate theoretical perspectives with individual theories, attempting to use the former to analyze historical or contemporaneous phenomena. For example, we are told that both Kant and Keohane are "liberal theorists" and that the "liberal perspective" is better than others at accounting for "NATO decisions on military strategy and force deployments" (50, 56–57). Small wonder, then, that the individual chapters cannot say much about which theories "explain"—or even are remotely relevant for describing—foreign policy phenomena.

Second, almost without exception, every chapter in the volume conflates theories of foreign policymaking and theories of international relations tout court. This begins as early as page 3, in which "theories in the foreign policy analysis literature" are transmuted three lines later into "theories in the subject of international relations." We thus end up with "an analysis of foreign policy behavior . . . in the tradition of the realist school of thinking" or an invitation to "explore the potentials of neo-realist foreign policy analysis" (pp. 123, 259). One result of this conceptual slide is that instead of studying the details of foreign policymaking phenomena (e.g., how thinking comes to be "grooved" how individuals become "hardball" politicians), the authors end up with the kind of high-order abstractions (e.g., "coordination," "supportive behavior") mentioned above.

Third, there is a pervasive confusion between metatheory and theory. This again begins at the very start of the volume, in which, only a few pages after having described the task of theory evaluation assigned to the contributors, one of the editors then goes on to cite the "agency-structure debate" as implying that it will be difficult, if not impossible, for theories of foreign policy analysis to be cumulated or integrated (16–20). To be sure, the other editor disagrees with this stance (285–7); and perhaps as a result of this split, several of the contributors feel obliged to interpret their empirical material as bearing on the "debate," rather than on particular theories of foreign policy making (e.g., pp. 200, 221). One does not have to be an adherent of "positivism" as variously defined by one of the editors ("a notion of truth . . . that can be approached," "one truth, one world," "data and their neutrality," pp. 12, 18) to see that the attempt at bringing data to bear on metatheory will result, at best, in the sort of jargonistic descriptions quoted.

This analysis should not be taken as implying that there is no possibility of developing useful—indeed, critical—theories of foreign policymaking. Considerable work is going on in this vein, some of it explicitly aimed at understanding Europe in the post–Cold War era. Much of this research, however, is "grounded" (as the term is used in sociological theory) and thus does not proceed in the top-down, metatheory-first fashion of this volume. Perhaps, in several years time, the editors could be persuaded to compile a second, more modest, collection.

Graduate Institute of International Studies, Geneva — David Sylvan

Finance and World Politics: Markets, Regimes and States in the Post-Hegemonic Era. Edited by Philip G. Cerny. Brookfield: Edward Elgar, 1993. 234p. $59.95.
States and the Reemergence of Global Finance: From Bretton Woods to the 1990s. By Eric Helleiner. Ithaca: Cornell University Press, 1994. 244p. $29.95.

The loosening of national regulations on banking, collapse of the post-World War II fixed exchange rate system, creation of new kinds of financial instruments and new capital markets, and sheer size of international transactions in currency, stocks, bonds, and other credit instruments have all led financial issues out of obscurity and onto the agendas of policymakers, analysts, and ordinary people. There is genuine concern over the impact of open financial markets on any government's ability to implement its policies, given the magnitude and speed of response from global financial markets. The response in the political science community has been a new wave of political-economic analysis of finance, from both a comparative and international perspective.

The books under review here—Eric Helleiner's *States and the Reemergence of Global Finance* and Philip Cerny's edited volume, *Finance and World Politics*, which includes work by Helleiner, Stephen Gill, Geoffrey R. D. Underhill, Peter Vipond, and Cerny himself—approach their subject matter in what might be called the "British" tradition of analysis (though not all are from Britain). The positivist research program typical of American academics is replaced here by an analysis characterized by its grounding in history. Helleiner's book focuses on the causes of the shift to an open international financial regime, while the Cerny volume focuses more on state responses to this openness. The authors attempt to grapple with what Susan Strange has described as the "structural" power of finance.

Helleiner's work should be read by anyone interested in the development of 20th century finance. Helleiner writes clearly and perceptively about state policy choice through a re-reading of the history of international finance, focusing particularly on the Bretton Woods monetary system. He uses Ruggie's concept of "embedded liberalism" to explain the shift from a system of open trade but closed capital markets following World War II to the current system of both open trade and open capital markets. As he convincingly demonstrates, the post-war order rested upon a fundamental compromise between international interests and the need to protect the welfare state. Complementing this, the Cerny volume focuses on the political effects of open capital markets and the undermining of that compromise, what Cerny labels the new "embedded financial orthodoxy."

Helleiner argues that the financial system had been an open one prior to World War II, and thus the current liberalization is not as unique as it is sometimes portrayed. Thus, he labels current trends the *re*-emergence of global finance. He demonstrates that the relaxation of controls reflects deliberate choices on the part of government leaders in the U.S. and Great Britain, and was not inevitable. This interpretation goes against the frequently found entrancement—especially in the popular press—with accounts which focus on modern info-technology as the overriding impetus towards change. The British and American governments, Helleiner argues, chose not to shut down the early Euromarket operations, although they had the policy tools nd power to do so. He argues they *chose* to promote capital mobility by relaxing controls; they were not forced by the mobility of capital to liberalize.

Helleiner's analysis is not necessarily new, based as it is on Ruggie's concept of embedded liberalism ("International Regimes," 1983). However, he provides detailed historical evidence to support this argument, and his research draws attention to the pre-Bretton Woods antecedents for global financial openness. His discussion of the negotiations leading up to the Bretton Woods agreement includes extensive information on the preferences of the major actors. He does all this in a fluid and accessible writing style that is enjoyable to read. One weakness is that the structure of the book is too evident in the repetition of the argument at the beginning and end of each chapter. However, it is an effective technique for ensuring the reader follows the complicated subject matter.

The central theme of the volume edited by Cerny is the globalization of finance in a "post-hegemonic" era, examined through historical-systemic and single country analyses. The authors start from the thesis that the transnationalization of finance limits the policy choices available to states, including the ability of any state to become hegemonic in the future. They discuss domestic political responses in the form of deregulation and re-regulation, and international responses in the form of inter-state negotiations over adjustment and management issues.

The decline of U.S. power is a thread that runs through this volume. Helleiner, in one of his two contributions, examines the extent to which the Japanese have undermined U.S. financial power, but argues that inherent weakness in the Japanese system, especially overregulation, prevents the yen from becoming a global currency. Cerny argues the U.S. has lost its position as the financial hegemon, with that role taken up not by Japan or Germany, but by the kind of haute finance described by Karl Polanyi in *The Great Transformation* (1944). Gill starts from a more Gramscian concept of hegemony, and explains the weakness of cooperation among the Group of Seven countries (G–7) as stemming not from declining U.S. power, but from an increase in the structural power of finance. Vipond argues that the operational capacity of states has been undermined, but that their design capacity remains viable in terms of setting the framework in which financial transnationalization will proceed. Underhill, in his analysis of the Uruguay Round negotiations over trade in services, points out that declining hegemony cannot explain this expansion of liberal policies into a new area and focuses attention on the domestic politics of change. Underhill contrasts with the other authors by not assuming that financial transnationalization has occurred already; instead, he views it as the probable end result of implementation of the Uruguay Round trade agreements.

This collection should not be viewed as simply another effort to debunk hegemonic stability theory, however. The authors are equally concerned with the issues of competitiveness that drive much of current state policy. Cerny's chapters in this book analyze the ways in which deregulation actually represented a new, a more market-oriented re-regulation. Vipond, in his analysis of the move towards a European financial area, presents integration as a method of contending with economic markets that no longer coincide with sovereign borders, and posits a strong role for individual firms in this process.

One weakness of both these volumes is that the authors rarely state exactly what they mean by the term "international finance." The structure and functioning of the monetary system and the system for creating and distributing credit are obviously linked, but different factors have driven change in each area. In fact, Vipond acknowledges that transnationalization needs to be differentiated across financial products. The system of credit tends to be fragmented among industries, instruments and jurisdictions. The monetary system has moved from a centralized to a decentralized structure, with authority resting in the hands of international institutions, domestic monetary authorities, and currency traders.

The authors reviewed here focus on the effects of financial globalization on the governing ability of advanced developed countries. Yet these states probably can cope with financial change better than any other states, and in fact, they have chosen to advance liberalization because they perceive advantages for them in it. Compare these books with, for instance, the collection edited by Stephan Haggard, Chung Lee and Sylvia Maxfield (1993) on finance in developing countries. In that edited volume, the authors analyzed financial repression, and described liberalization in terms of the domestic and international interests that would benefit from the changing financial order.

It is difficult to do justice in such a short review to the complex and varying approaches presented by these authors. They provide a solid overview of the underlying causes of the transnationalization of finance, and the issues of accountability and control it raises. Their research contributes substantially to our understanding of the modern world economy, in which finance is coming to dominate trade in the global distribution of economic activity. It is an area ripe for further research.

University of Maryland, College Park VIRGINIA HAUFLER

Gorbachev, Reform and the Brezhnev Doctrine: Soviet Policy toward Eastern Europe, 1985–1990. By Glenn R. Chafetz. Westport, CT: Praeger, 1993. 156p. $47.95.

Among the numerous monographs and innumerable articles inspired by Gorbachev's revolutionary policy toward Eastern Europe, this book by Glenn R. Chafetz ranks somewhere in the middle. It argues rightly that only an account considering all three levels of analysis—Gorbachev's personality, domestic bargaining in the Soviet elite, and international pressures—can do justice to the historic decision to abandon the Brezhnev doctrine. In addition, the author—correctly, in my view—concentrates on the change in ideology that made it possible. With the advent of mutual nuclear deterrence, Soviet control over Eastern Europe no longer contributed to the regime's military security. Still, Moscow propped up these wobbly regimes with enormous military and economic resources out of fear that their collapse would delegitimate communist rule at home. By the mid-1980s, however, the ideology had lost its luster: the regime's inability to adjust to the demands of new interdependent economy and its failure both to rack up new international successes and to meet the expectations of a more urban, more educated population undermined both its overt claims to be a revolutionary power and the less heroic obligation undertaken by Stalin's successors to improve living standards at home. When Gorbachev came to power, recognizing the need to change the regime's ideological base, he diminished the role of class struggle in the Soviet analysis of international and domestic politics and so removed the leadership's reasons for resisting political reform in Eastern Europe.

Following the Sovietological method, Chafetz supports this argument with a review of the debates on Eastern Europe in the Soviet press and a detailed account, based on published sources, of the political battles that informed these debates. His research for the Gorbachev period is thorough, and his judgment solid. Unfortunately, though, his method shares the tendency of other Sovietology-in-transition works to privilege description over explanation and reflective interpretation. The organization of the book exacerbates this tendency by isolating the international, ideological, and domestic political aspects of Soviet decision making in separate chapters. For example, the chapter on Gorbachev's consolidation of power concentrates on the general secretary's manipulation of personnel and institutions at the highest levels of the Soviet power structure but does not give the reader much insight into how larger international and domestic forces intruded into this process.

Moreover, Chafetz frames his history within established notions of state interests, ideology, and legitimacy, rather than placing these concepts at the center of his analysis. The result is a static account that raises only briefly the crucial questions of the period regarding the international sources of ideological change, the linkage between foreign policy and domestic legitimacy, and the reasons why some leaders choose reform over retrenchment in response to international pressure. For example, the author makes the excellent point that state interests are "amorphous and changeable," that "consensus both defines and is defined by the national interest" (p. 11). His emphasis on ideology, in fact, requires that he examine how the Soviet leadership defined and redefined the national interest in the Gorbachev period. But rather than developing this argument, he undercuts it by offering his own static and somewhat conventional definition of Soviet interests as consisting of the physical security of the Soviet state and its people; the promotion of a vigorous national economy to ensure the welfare of the populace; and the authority of the Communist party. For the most part, in fact, his account deprives ideology of an active role in shaping the leadership's cognitive processes. If the leadership must uphold the ideological premises of Marxism–Leninism, he seems to argue, it is only to realize the third element in his definition of state interests—maintaining the authority of the Communist party. He notes little evolution in the regime's essential ideological premises from Stalin to Brezhnev and dismisses the changes that did take place before Gorbachev as tactical ploys to expand the leadership's room for maneuver in pursuing the more important interests of protecting Soviet borders, improving the Soviet economic performance, and mollifying the Soviet people. This argument is defensible, of course, but it should not be assumed. Stalin had a very different notion of who counted among the Soviet people than his successors did: indeed, he executed many Soviet citizens, even Party members, as enemies of this people. Such static definitions also reduce Gorbachev's political and ideological reforms to a pragmatic effort to improve the economy, not as a process of cognitive revolution. But Gorbachev's recognition of individual interests outside the class struggle not only reflected a change in the means Brezhnev used to promote the welfare of the Soviet people; it also forced him to reconsider who the citizens of the Soviet Union were (proletarians? Soviets? Russians? Georgians?) and hence what their welfare entailed. By extension, he also had to rethink how, when push came to shove in 1989, he would treat the citizens of Poland (for example) in relation to the Soviet regime—as proletarian brothers and sisters, as subjects of a Soviet empire, or simply as citizens of another sovereign state?

In short, the book provides a great deal of useful information regarding Soviet policy toward Eastern Europe but offers relatively little to audiences with broader interests. It is very much a victim of time. It utilizes an older method inappropriate to a challenging new era and applies static concepts to a dynamic process. But the damage does not end there. Memoirs and other material from the period recently made available to Western researchers will diminish the significance of the book as history, even though the essential moments of Chafetz's account hold up pretty well. Finally, the book seems to have been produced in a hurry, particularly the second chapter, which could have been edited more carefully.

Bates College James Richter

The Two Faces of National Interest. By W. David Clinton. Baton Rouge: Louisiana State University Press, 1994. 278p. $35.00 cloth, $15.95 paper.

This is by no means the first attempt to salvage the concept of *national interest* (if appears in nearly 1,200 titles in the University of Michigan Mirlyn system), but it is certainly one of the best. I regarded the concept as a smoke screen until I began reading this serious, competent, and intelligent piece of political analysis. The study is divided into two parts. In part 1 we are treated to a thorough and balanced examination of the various attacks on the concept, and in part 2 the author produces a spirited defense of its utility and its relevance, particularly in the context of U.S. foreign policy since World War II.

As we move into a global environment that is appreciably different from what it was over the past half-century, this study could make a valuable contribution to the reformulation of foreign policy in the United States and around the world. Indeed, one of the virtues is that Clinton is no chauvinist. In the best tradition, he reflects Kennan's crucial reminder that other societies have—and pursue—interests of which we need to be aware and give credence and legitimacy. Nor does he assume that all is eternally fixed. Not only do we inhabit an evolving global system, but the role of the state vis-à-vis other forms of social organizations is once again in a state of flux and uncertainty. He is fully aware of the empirical reality that it has been a long time since the dominant social unit has been the nation-state. Most of the mature states in the world today are multinational, and many nationalities are in a sort of diaspora, spread across several states.

Yet another major strength is his reluctance to define the national interest in the primitive terms of our self-styled "realists." Rather, Clinton sees preservation of the state, its territorial integrity, and regime survival as the core of a state's vital interest, with the maximization of autonomy and minimization of foreign constraints a parallel priority. My interpretation is that he understands power in the broader sense and treats industrial and military capabilities as largely instrumental, even suggesting that domestic support from elites and attentive publics may count for more than force in maintaining autonomy vis-à-vis the larger system. Further, I read him as appreciating what Wolfers called milieu goals: the desirability of creating and strengthening regional and global arrangements and institutions that might well call for short-run constraints on autonomy in order to head off middle-run threats and thus be able to enjoy greater scope and latitude further down the road.

It should be evident that I find the ethical tone and the pragmatic thrust of this study entirely attractive. Put another way, one could be quite comfortable were this particular Clinton playing a central role in formulating U.S. foreign and military policy in the current epoch. His premises are reasonable, his arguments logical, and his expectations realistic. Having said this, however, by the time I had read the first-rate case studies of "national interest in practice," all of my original reservations regarding the concept began to reappear.

Bluntly stated, the concept is nearly useless for guiding us in the choice of policies in the future or as a yardstick for evaluating those that were followed in the past. There are no consistent criteria beyond the vague and the abstract. It does not even offer the banality of the Golden Rule or clues as to when to go with honesty and when to practice deception, not to mention guidance as to when military force is best threatened and/or used and, if so, to what extent. As the case studies reveal, the criteria of interest lead to interventionism on one occasion and distancing on another, to urgency here and patience there, to a concern for individual rights in one case and to "national honor" in another, to hanging tough against one rival and to showing flexibility in another, to economics over security here and vice-versa there. The explanation should be clear: the concept is nearly content-free; or (as Clinton comes close to saying in his conclusion) it is what those in control define it to be in a specific context.

The problem lies as much in one of the two words as in the other, even if we used the French equivalent *raison d'état*. As to *interest*, this is no better than *goal, purpose, objective, preference,* or *utility*. Looking back on U.S. and Soviet policies during the long, dangerous, and costly armed rivalry known as the Cold War, the citizens (not to mention the elites) on both sides were not only treated to the most specious arguments packaged under the national interest or vital interest label but, worse yet, were persuaded by these arguments. As a result, the world teetered on the brink of nuclear catastrophe but also ended up poorer by far than it needed to be. Further, one could make a good case to the effect that the superpower misallocations at home and interventionism abroad contributed appreciably to many of the woes that confound us today.

The other word *national* is no better, not only because the key actors are not nations but also because the notion of *any* collective interest must be suspect; that is, every policy and every policy outcome falls differentially on the different strata and sectors of the collectivity. Some get rich from military spending, and some remain poor; some watch nuclear weapons tests on television, and some develop cancer and leukemia; some get free college educations via ROTC, and some get the abuse of boot camp; some conduct wars from the safety of a heavy cruiser, and some kill and die in the jungle or the trenches; some are lionized and legitimized for their mindless loyalty to the regime, and some are marginalized, ostracized, hounded from their jobs, or even imprisoned for their skepticism and willingness to speak out.

Thus, despite the scholarly virtues of this study (and the policy sophistication of the author) it must, in the end, be deemed a failure because it helps to legitimize a concept that is intellectually vacuous and politically mischievous. One might hope that Clinton would atone for these scholarly sins by turning his appreciable talents to a follow-on assignment. Several come to mind, but one that I like would have him take a range of foreign policy decisions that were sold under the "national interest" label and then go on to evaluate the consequences of those decisions in terms of their costs and benefits to various groups of human beings at home and abroad. In doing so, he might bear in mind that we were willing to blow up most of the world for access to Berlin but agree today with a former secretary of state who thinks it "insane" to risk a handful of troops to bring down a brutal, murderous usurper in our own backyard.

University of Michigan J. DAVID SINGER

Images and Intervention: U.S. Policies in Latin America. By Martha L. Cottam. Pittsburgh: University of Pittsburgh Press, 1994. 226p. $49.95 cloth, $19.95 paper.

Why has the United States repeatedly used military intervention or covert operations in Latin America, rather than other means of influence, such as diplomacy and accommodation? Cottam argues that the image held by U.S. policymakers of Latin American states as dependents has distorted their perception of these countries' political situations and predisposed them to resort to coercive methods.

Defining *image* as a complicated bundle of cognition organized into types of states, Cottam does not relate it to the standard constructs such as *schemas* or *belief systems* found in the political psychology literature—although her notion of image resembles a schema, a cognitive structure that organizes stored knowledge about a concept. Because she does not use standard theoretical concepts or cite much relevant psychological literature, her book does little to advance knowledge about the role of cognitive structures in foreign policy decision making.

On the other hand, Cottam's study remedies the lack of systematic analyses of beliefs and values in studies of U.S. policy toward Latin America. As evidence for the influence of images and perception, she examines well-known and some less familiar cases of U.S. intervention in Latin America, including the Central Intelligence Agency's involvement in the overthrow of the Guatemalan government in 1954, policy toward Fidel Castro up to and including the Bay of Pigs, covert operations against Salvador Allende in Chile, use of economic sanctions in response to nationalizations in Peru in the late 1960s and early 1970s, and varying U.S. responses to the Nicaraguan revolution and guerrilla activity in El Salvador.

Cottam tries to use images to explain patterns in U.S. policy toward Latin America, such as the use of coercion or the selection of slow-version-quick-acting policy instruments. This is a laudable but perhaps overly ambitious objective, because even if officials support policies consistent with their beliefs, they may be influenced by other considerations, such as domestic politics or *which* agencies are behind a particular proposal when they make the final decision. Only the president or perhaps the secretary of state can be guided solely by their convictions in deciding foreign policy questions; but in most cases, higher-level decision makers were not interested or engaged by events in Latin America and lower-level bureaucrats set policy.

Within the framework of general beliefs about foreign policy and America's role in the world, officials at lower levels make incremental decisions that add up to policy. That these beliefs are usually shared within an administration makes it difficult to refute the argument that both U.S. policy and prevailing images were determined by external factors, such as the bipolar rivalry with the Soviet Union or the need to preserve a favorable environment for capitalism.

Thus, her argument is most compelling when there were varying images or worldviews implying radically different policies, as in the Carter administration, where human rights advocates favored nonintervention in the Sandinista revolution in Nicaragua, modified cold warriors supported reform until the Soviets appeared to be involved, and cold warriors favored using force to prevent a Sandinista victory.

Showing that a foreign policy decision was consistent with policymakers' images is insufficient grounds to make a causal argument if the decision was also congruent with other variables, such as domestic politics, economic interests, or security concerns. Expressed beliefs often rationalize or explain behavior chosen for other reasons. Cottam does not give sufficient attention to alternative explanations for U.S. intervention, such as U.S. policymakers' concern to deter expropriation of U.S. companies, anticipation of right-wing political attacks about "more Cubas," or the desire to have reliable client regimes within the American sphere of influence.

Even if we accept that U.S. security was rarely endangered and that policy was driven by subjective perceptions, Cottam does not show that the dependent image, rather than anticommunist beliefs provided the lens through which U.S. policymakers viewed various Latin American countries. United States officials viewed virtually all countries as vulnerable to communist domination at one time or another, including Western Europe after World War II.

Invoking images to explain policy requires looking beyond the public rationalizations given by governmental officials to justify their policies. Further, as Cottam notes, one may not use the same verbal statements to infer beliefs and policy preferences without engaging in tautological reasoning, but she does not consult enough additional sources to portray the leading decision makers' beliefs.

Cottam's book raises a larger prescriptive issue. Cognitive psychologists have found that categorization is integral to perception of the world in all of its complexity. Categories enable people to code, classify, organize, store, and retrieve incoming information. Further, we know ourselves by differentiating ourselves from others; identifying the defining attributes of an outgroup is essential to forming one's identity. The very categories that allow us to process information efficiently, however, lead to error when individual examples are assimilated to the prototypical members of a category. If the dependent image is in error, then with what category should it be replaced? It is not realistic to recommend, as Cottam does, that policymakers examine each Latin American country in terms of its own history, political development, and culture. Other problems are always on the agenda, and presidents have a limited amount of time to spend on each foreign policy issue. As George Bush decided what to do about Manuel Noriega in Panama, Soviet hegemony in Eastern Europe collapsed and with it the bipolar distribution of power that preserved stability in Western Europe for a half-century. Perhaps the answer lies in better use by presidents of expert advice, but experts are often unaware of the larger context and competing values. There is no substitute for informed judgment by higher-level decision makers.

University of California, Los Angeles DEBORAH WELCH LARSON

The United States and the European Community in the 1990s: Partners in Transition. By Kevin Featherstone and Roy H. Ginsberg. New York: St. Martin's, 1993. 284p. $65.00.

The dynamics of European integration have always been affected by external, as well as internal, factors. Not only were member states reluctant to surrender aspects of their national sovereignty to supranational authority and coordinate their economic and monetary policies, but they pursued distinct foreign policies toward important nonmembers, such as the United States and the Soviet Union. At the same time, the perils and uncertainties of the Cold War forced the West Europeans in the decade following World War II to accelerate their efforts at integration, often prodded by the United States, which sought a secure political and economic foundation from which to conduct its policy of containing the Soviet Union, as well as the newly established Federal Republic of Germany.

The authors of this book are persuaded (correctly, I believe) that the United States–European Community partnership has arrived at a critical juncture, which will force both parties to adjust to shifts in their relative roles. The United States must come to terms with its changing global position and its inner-directed political grievances; and the European Community must accept the burdens and opportunities of leadership in a Europe no longer divided, with its Eastern half demanding economic, political, and ultimately psychological association with its prosperous Western half, sustained by security arrangements that guarantee peace in the era of postcontainment. The issue of deepening versus widening is central to the future direction of the Community and thus central, as well, for its future relations with the United States.

The authors of this book set themselves three major tasks: to demonstrate the intricate web of multiple types of interdependence that connect the two pillars of the transatlantic community; to examine the political, economic, and social constraints on both sides that stem from this mutual dependency; and to determine to what extent the current management of United States–European Community (U.S.–EC) relations is deficient (and capable of reform) in light of changed conditions. The core concept around which the book is organized is that of interdependence, applied to the political, economic, and social dimensions of U.S.–EC relations. The authors argue that the totality of U.S.–EC relations is best comprehended by the notion of complex interdependence but that specific aspects of the relationship (e.g., trade disputes and security issues within NATO) are best examined with the notions of realism and neomercantilism. (Noticeably absent is the concept of integration.) The first 70 pages, which explicate this compound methodology, are an unnecessary academic digression, which fortunately proves harmless in the long empirical portion of the book, which examines in some detail the existing interdependencies in the political, economic and social realm. Here the authors are at their best, clearly delineating how the extensive fabric of relations that stretches across the Atlantic was woven over the past decades, where it has become tattered, and where it shows remarkable resiliency.

Early on in their analysis and intended as a justification for undertaking it, the authors note that "over the past decade, there has been a disquieting dearth of literature on U.S.–EC relations" and that "there is a pressing need for several new volumes on the subject given its ever-widening complexity and the recent epic changes within and around the North Atlantic" (p. 3). Perhaps. But there are reasons why the topic of U.S.–EC relations, perceived as an essentially bilateral set of issues, has not been the focal point of inquiry. The Community has, with all its integrative success, remained a grouping of nation–states; and by failing to elevate the notion (and problems) of integration to a level of importance similar to that accorded to interdependence, the authors miss the opportunity to play their conflicting regional and global dynamics against one another. The West Europeans themselves were always acutely aware of this double restraint on their national autonomy, and it affected their relations not only with the Community but with the United States. From the beginning, members of the EC were determined to retain a measure of economic and monetary independence, the freedom to organize their national economic life according to the principles and values they cherished, even as they exploited the opportunities and accepted the obligations of their participation in an interdependent global economy and an integrated regional West European economy. The demands of global interdependence (or, more narrowly in this book, U.S.–EC relations), of regional integration, and of national self-assertion required of all parties a complicated set of calculations, a series of economic triangulations among conflicting political and institutional requirements. This is why an analysis such as the one proposed by the authors, which stresses the bilateral institutional, rather than the multilateral-national nature of the U.S.–EC relationship, cannot persuade fully. Among other shortcomings, it ignores the issue of deepening versus widening of the Community's institutions and membership, which spills over into the area of security arrangements within NATO and will affect U.S.–EC relations in profound ways. It is not sufficient to say, when the authors return in their concluding chapter to the analytical problems of their study, that "in the political domain, the multidimensional nature of Europe's international relations creates a profound analytical complexity," that "for the EC, foreign relations increasingly involve both national governments and EC institutions," and that "complex and varied political channels [within the EC] involve different types of actors, issues and interests" (p. 245). The issue is how to unravel this analytic web, not merely state it.

It is true, of course, that "many on both sides of the Atlantic are now asking what is left to U.S.–EC relations after the cold war" (p. 250). That is indeed the central question, and what is required (but the authors fail to provide or invite it) are reflections on the deeper strategic tides that carried and sustained the partnership between the United States and Europe in the era after World War II. Such reflections would need to assess the relative weight of the forces of economic interdependence, which seem to portend an enduring, if at times contentious, U.S.–EC relationship, and of the forces of geostrategic "realism," which have formed the historic American interest in a stable, balanced European political order and propelled the United States to restore it three times in this century: in the Great War, in World War II, and the Cold War. Should the United States perceive a self-equilibrating European order (which would permit a less intense American engagement) and should an enlarged or deeply integrated European Community perceive its fortunes as less dependent on the United States, the common interests that sustained U.S.–EC relations from the end of World War II to the end of the Cold War would diminish and rest on less secure foundations.

University of California, Santa Barbara WOLFRAM F. HANRIEDER

Human Rights in the New Europe: Problems and Progress. Edited by David P. Forsythe. Lincoln: University of Nebraska Press, 1994. 279p. $40.00.

The "new Europe" examined by 12 contributors to this volume is limited to East-Central European states in transition since 1989. Three of the authors from the former Czechoslovakia focus primarily on that state's history and political reform prior to its 1993 division. The edited volume does not consider atrocities in the Balkans and gives priority to political and civil over economic rights.

Jack Donnelly's introductory chapter on human rights in a new world order provides a basic overview of how international law and institutions have evolved since World War II. Despite post–Cold War liberalization, he faults Eastern European prosecutors for violating due process in punishing political enemies and the United States for a moral failure to pursue human rights beyond elections and free markets. Economic ills brought down Marxist planners, and democratic reformers' inability to deliver improved living standards now endanger democratic leaders who respect human rights.

Five chapters on rights development in Eastern Europe focus on political parties, constitutions, legislatures, and executives in Poland, Hungary, and Czechoslovakia. Proportional representation is the norm in complex systems modeled on continental, rather than American, institutions. Election devices to assure minority representation have great theoretical appeal but create major difficulties in implementation.

Long before the post–World War II Human Rights movement incorporated self-determination into the International Bill of Rights, Eastern Europeans struggled to establish stable national borders. Nearly a century after the Hapsburg's departure, minority Slovaks have demanded national independence from the majority Czechs. Despite Czechoslovakia's celebrated commitment to constitutional guarantees of civil liberties and local political autonomy, Slovak nationalist self-determination was the paramount human right.

None of the contributors notes Yugoslavia's leadership of the prolonged UN effort to draft an international human rights standard protecting minorities. The Yugoslav delegates' belief in their system of group accommodation proved so misplaced that others may despair about the prospects of political engineering with alien institutions. While recognizing the growing importance of "intermestic" issues, Forsythe repeatedly concludes that national factors are more significant than international regimes in assuring protection for civil liberties.

Forsythe nevertheless concludes that foreign policy pressures and international human rights institutions have had some influence in promoting political reform. Republican Representative Doug Bereuter of Nebraska describes the Reagan and Bush administration pressures on Eastern European communists and the subsequent support for new political parties and labor organizations. His discussion presumes familiarity with the National Endowment for Democracy and left me wondering whether recipients questioned U.S. support for opposing parties contesting the same seats.

In contrast to Bereuter's essay on U.S. altruism, Raphael Zariski's review of West European foreign policies recognizes that political and economic interests rival human rights concerns. In order to reduce Cold War tensions and to promote trade, Europeans preferred quiet diplomacy to the Americans' confrontation of communist adversaries. Mark Gibney also identifies the problems for Western Europeans when refugees fleeing communism joined Third World "guest workers" in competition for jobs with angry voters. Economic assistance from the European Community, especially Germany, advanced the national interest of reducing the number of asylum seekers by improving economic stability in the east.

In a particularly informative chapter on European multilateral institutions, Forsythe concludes that political democratization must precede the economic benefits of admission to the European Community. The considerably larger Council of Europe promptly legitimized the new governments of Poland, Hungary, and Czechoslovakia by granting membership. Valuable reference lists in the appendix identify the numerous Eastern governments adhering to major international human rights instruments from 1989 to 1993. Forsythe details the extensive multilateral pressures that predated the Soviet collapse but acknowledges the impossibility of determining how much international regimes contributed to the positive outcome. Nor can the social scientists describing change in the new Europe predict the future prospects for rights protecting institutions, as illustrated by the unanticipated transformation of Yugoslavia and Czechoslovakia.

The most idealistic human rights activists aspire to promote human rights by educating for a civic culture that transforms political institutions. Václav Trojan reports on the growth of private organizations in Czechoslovakia but worries about their capacity to overcome deeply rooted ethnonationalism and apathy resulting from four decades of communist control. Richard Claude suggests that constitutionally mandated human rights instruction in the Philippines is a valuable model for assuring that law enforcement personnel understand the rule of law.

Human rights ideals demonstrated enormous subversive power in the democratic revolutions of the 1980s. Attempts to institutionalize human rights education in formal school curricula and police training academies complement the informal networking facilitated by computer technology and satellite transmissions beyond reach of state censors. Optimistic forecasts of a global consciousness and world rights culture seem premature, but realizing the ideal requires faith that humans can achieve that valued goal.

Forsythe's edited volume reveals the confusing breadth of human rights scholarship in the 1990s. Historians, sociologists, philosophers, political scientists, and public officials offer differing perspectives on current political developments, national culture, historical forces, and law that embrace topics as diverse as self-determination, economic development, and civil liberties. This broadly titled collection includes both essays and documented scholarly analysis primarily about recent political reforms in three former communist states of East-Central Europe.

The editor has included ample cross-references to link different chapters but is still unable to synthesize the findings in a way that significantly advances scholarship on either the region or international human rights. The work offers a useful snapshot of several European states in transition and makes no pretense that its contributors have discovered explanatory variables that explain past democraticzation or allow prediction of Eastern Europe's uncertain future.

University of Cincinnati HOWARD TOLLEY, JR.

Leadership Style and Soviet Foreign Policy: Stalin, Khrushchev, Brezhnev, Gorbachev. By James M. Goldgeier. Baltimore: Johns Hopkins University Press, 1994. 169p. $35.00.

In *Leadership Style and Soviet Foreign Policy*, James Goldgeier develops a simple and useful model for interpreting foreign-policy decisions. Most world leaders rise to prominence, he posits, as a consequence of having prevailed in a series of domestic political struggles. Schooling in domestic politics, and particularly those "vivid and salient" experiences associated with major successes, gives rise to *schemas* (austerely defined as simplifying assumptions about the way things work) that reinforce certain kinds of patterned political behavior and bargaining techniques. Applied later on to foreign policy encounters, these patented bargaining styles often lead to misperceptions, flawed choices, and undesired outcomes.

The author illustrates his model with four case studies and accompanying secondary cases. Soviet decision-making under Stalin is explored during the Berlin crisis of 1948, with the negotiations at Teheran, Yalta, and Potsdam as a secondary case. The Soviet dictator's "good cop/bad cop" approach to negotiation, presumably perfected during the post-Lenin succession struggles of the 1920s, includes striking the "typical role as the grand conciliator" while subordinates are assigned the task of asserting what is in essence a coercive strategy. Khrushchev's blustering diplomacy is explored during the Cuban missile crisis, as well as during the Berlin crisis of 1961. In these cases Khrushchev's preference for strong personal commitment, a confrontational style and strategy of the fait accompli, methods that were applied with success as the Soviet reformers imposed his domestic political agenda during the 1950s, give way to conciliatory outcomes under strong international pressure. Brezhnev's bargaining style is perceived to rest on a strategy of postponed commitment and personal accommodation perfected while climbing to the top of the Soviet collective leadership. These traits are represented as the key to the Soviet diplomatic approach during the Middle Eastern war of 1973, as well as during the secondary case of the Czechoslovak crisis of 1968. Finally, Gorbachev's early domestic successes are attributed to a chronic avoidance of fixed commitments and a tendency to shift positions rapidly in search of viable consensus, a bargaining style that he is perceived to have applied during the negotiations leading to the unification of Germany in 1989–1990 (the Reykjavik negotiation is presented as a secondary case).

All of the cases explored by the author end in foreign policy failures. Stalin's conciliatory public gestures are not sufficient to deceive the West with the prize of Berlin at stake; Khrushchev's bluster must give way to conciliation in face of the "overwhelming external threat" of nuclear war; Brezhnev's tendency to accommodate and postpone choice are overwhelmed by the "major change in the external environment" created by the defeats of the Syrian and Egyptian armies; and Gorbachev's temporizing, without the coercive threat of resignation that had served him well domestically, causes the Soviet Union to miss the chance for a compromise arrangement on German unification. In every case, standard approaches anchored in cognitive rigidity and fixed dispositions derived from prior domestic success lead leaders and their countries astray.

The author's argument rests upon the premise that, although state behavior may be importantly constrained by the structure of the international state system or other essentially impersonal variables, decisive political choices are ultimately made by individual leaders, whose perceptions and misperceptions are vital to explaining outcomes. The argument works fairly well in the case of Soviet decision-making, notoriously centralized and often subject in fact to the whims of the all-powerful general secretary. The logic of using Soviet foreign-policy decision-making as the subject of study is however never justified by the author in a systematic way. Nor are the special conditions that affect foreign-policy choice in the Soviet context, beyond those occasioned by the bargaining style of the leader, explored with any kind of rigor. The sizable literature on Soviet foreign-policy process, for example, is under-used and under-referenced. As a result the case studies illustrate the thesis but do not powerfully confirm it. The premise is credible, and it is presented clearly and elegantly, but a number of important conditioning variables are left out of the picture.

The case studies themselves, while sufficient to make the author's point, are also rather slim. The secondary cases, usually treated in the space of one or two pages, do not add anything more than a superficial reconfirmation of the basic argument and might have been dispensed with altogether. The book as a whole is in fact shorter than it need be, and perhaps shorter than is required to be fully convincing. The author has read widely and can speak authoritatively about the events he describes. The most recurrent and influential references are nonetheless to standard accounts (Hannes Adomeit on the Berlin crisis, Raymond Garthoff on Cuba, Henry Kissinger's memoirs on the Middle East crisis, Gorbachev's own statements on Germany unification) and do not reflect a wide range of diverse interpretations. New archival materials are also left untapped. The case study approach that has been adopted poses special demands if an acceptable level of historiographical rigor is to be maintained. *Leadership Style and Soviet Foreign Policy* is historically sound, but the author's thesis would be strengthened by a fuller treatment of the case studies, including a confrontation with events and interpretations that conflict with the points he wishes to confirm.

Goldgeier makes a solid and original contribution to our understanding of the complex ways in which conceptual predisposition and domestic politics influence foreign-policy choice and crisis diplomacy. His study would be well-served by a more forthright coming to terms with the specificity of the Soviet decision-making environment, and by a more ambitious approach to the case studies, including a less narrow focus upon illustrating the thesis, at times at the expense of meaningful nuance and complexity.

Johns Hopkins University
Bologna Center
R. CRAIG NATION

Ideas, Interests, and American Trade Policy. By Judith Goldstein. Ithaca: Cornell University Press, 1994. 268p. $39.95 cloth, $13.95 paper.

Ten years ago, it was not fashionable to study the role of ideas, as opposed to interests (power), in international politics. Under the influence of realist epistemology, ideas were dismissed as mere ideology—rationalizations of interests or, worse, illusions and propaganda.

Judith Goldstein's book, along with Peter Hall's *Political Power of Economic Ideas* (1989) and Ernst Haas' *When Knowledge Is Power* (1990), among others, is part of a recent effort to reverse this fashion. Goldstein argues that particular American trade policies from the early republic to the Reagan years cannot be explained satisfactorily solely in terms of international structural or domestic economic interests. In three periods in particular, ideas defined both in (causal beliefs as to which economic policies can best achieve preferred interests and the laws and institutions embodying such beliefs) are crucial explanatory factors. In the period after 1860, protectionist policies persisted even after objective interests, reflected in America's growing competitiveness and export orientation, should have dictated a shift to more open-trade policies. Again in the 1930s, American trade policies shifted gradually toward more open trade even though structural pressures, such as world depression and economic nationalism, should have produced more protectionist policies. Finally, in the 1970s and 1980s, ideas embedded in free-trade laws and institutions kept U.S. trade policy open even as the decline of U.S. power should have brought about a more protectionist trade policy stance.

Prior to 1860, no consensus existed as to whether free trade or protectionist strategies best served group and national interests. In this period, trade policy largely mirrored direct regional (North vs. South) and economic (manufacturing vs. commerce and agriculture) interests. After 1860, however, a new Republican party seized upon a high-tariff, or protectionist, policy to unite a broad coalition of free-labor (antislavery) and high-wage (industrializing) interests. This policy persisted until 1930 despite the growth of free-trade theories in academic circles and the growing interest and capability of American manufacturing industries to export to foreign markets.

The Great Depression finally created the massive uncertainty as to which new ideas best access and influence policy choices. Incremental innovations in the Trade Act of 1934 authorized for the first time reciprocal opening of U.S. markets and delegated power to the executive branch to negotiate tariff reductions. Free-traders, such as Cordell Hull, gained access to policymaking and, particularly after the war, shifted policy decisively toward freer trade. These policies then persisted beyond the 1970s, after America's relative economic power declined. Despite the expansion of trade laws to remedy fair (e.g., escape-clause provisions) and unfair (e.g., Section 301) trade practices, U.S. trade policy, Goldstein argues, remained open and is likely to continue to do so as long as significant alternative trade policy ideas do not emerge.

Goldstein's study represents another important step out of the quagmire of structural and class interest analysis that dominated political science research during the Cold War period. To write, as she does, that "it is not markets but ideas that establish the rules of the game" (p. 238) puts economic forces into an entirely different perspective than realist or Marxist accounts have done. Unfortunately, her research design in this study is too narrow to make the case fully convincing. By focusing primarily on ideas as causal beliefs and institutions, she ignores two other levels at which ideas significantly affect policy choice and academic research: ideas as epistemological worldviews and ideas as normative values. (She recognizes the role of ideas at these levels in a later volume with Robert Keohane, *Ideas and Foreign Policy* [1993].)

Goldstein accepts realist epistemology. She does not want to fight about the centrality of material interests in political analysis but is interested only in the efficacy of particular strategies for achieving these interests (p. 9). If the chosen strategy achieves material interests, however, outcomes are overdetermined. We have no way of knowing whether the choice of strategy or the objective interests themselves produced the result. If, on the other hand, strategies lead to outcomes that contradict material interests, then ideas may matter. Goldstein claims that protectionist strategies contradicted American economic interests in the late nineteenth century and that free-trade strategies did the same in the early 1930s and again in the 1970s and the 1980s. But none of these claims is substantiated by measures of objective (material) interests.

Follower-leader theories of economic development suggest that followers benefit from protectionist strategies, while leaders benefit from freer trade policies. The United States was a follower behind Great Britain in the late nineteenth century. According to Baumol and colleagues in *Productivity and American Leadership* (1989), the United States passed Britain in overall productivity levels only around the turn of the century. Protectionist policies after 1860 therefore were not inconsistent with U.S. material interests. Similarly, the United States was a leader by the early 1930s and after World War II. Although its power declined relatively after the war, this decline stopped in the 1970s and 1980s (Nye, *Bound to Lead* [1990]). And the United States, even in the 1990s, remains comfortably ahead of its competitors in both overall and manufacturing productivity (see McKinsey and colleagues' *Service Productivity* [1992] and *Manufacturing Productivity* [1993]). Free-trade policies in the 1930s and 1990s, therefore, are not necessarily inconsistent with America's material interests. Goldstein does not offer sufficient evidence in any of these periods to establish the point at which America's "objective" economic interests may have shifted from follower to leader. If that point occurred during the first quarter of the twentieth century, ideas account for relatively little of the shift from follower status and protectionist policies after 1860 to leader status and freer trade policies in the 1930s, except perhaps for a brief transition period in the 1920s.

Second, the focus on causal beliefs ignores the role of ideas not just in achieving outcomes but also in evaluating them. As Goldstein points out, both protectionist and free-trade policies were consistent at various times in American history with prosperity, that is, the achievement of material interests (p. 70). This was also the case in the period of the 1980s, when U.S. trade policies were more protectionist than in the 1950s and 1960s but U.S. economic growth was comparable to that in the earlier decades. Is the choice of trade policy then purely ideological, that is, unrelated to outcomes (prosperity)? Probably not. Material outcomes are simply being evaluated on terms other than, or in addition to, material interests. After 1860, the normative idea of antislavery or free labor came to justify high-wage and protectionist policies even when these policies later contradicted commercial interests (though, as I have noted, the evidence of the latter is not persuasive). Similarly, after 1970, the normative ideas of freedom and protecting the free world justified freer trade policies even after (as Goldstein argues but does not demonstrate) U.S. economic power declined and should have motivated a more decisive shift to protectionist policies. The nonmaterial or normative ideas of nationalism and manifest destiny may also account for the shift from pure protectionist (eschewing both imports *and* exports) to mercantilist trade policies after 1897, when the United States pressed harder for foreign markets but continued to protect home markets.

The focus on ideas, as well as interests, is long overdue. But to achieve new insights, explanations based on ideas cannot avoid the debate about the centrality of material interests in political analysis (as Goldstein does, p. 9). It will have to confront explanations based on power and show under what circumstances ideas explain more than interests based on power.

George Washington University HENRY R. NAU

Political Opposition and Foreign Policy in Comparative Perspective. By Joe D. Hagan. Boulder: Lynne Rienner, 1993. 274p. $40.00.

Second-image analysis has recently staged a comeback in the study of international relations. The second image, focusing explanatory emphasis on the political structure of states, stands between the image of human nature and individual behavior on the one side and the anarchy of the international system on the other in the analytical trifocals through which scholars have come to view and interpret the international political environment. Kenneth Waltz not only originated this image terminology more than 30 years ago in *Man, the State, and War* (1959), but his penetrating theoretical critique of the second image was largely responsible for its long absence from serious scholarly attention. One thus might have expected that any resurgence in second-image analysis would emanate from new theoretical insights. In fact, the second-image has benefitted more from empirical advances than theoretical ones, a trend most obviously exemplified by the growing body of historical evidence supporting what has come to be known as the democratic peace.

Joe Hagan's book represents a welcome theoretical contribution to the second image, for it digs beneath the shiny veneer of institutional structures to uncover and highlight the rich texture of domestic politics lying just below the surface. To bring this texture to the fore, Hagan focuses on two key concepts: the *political regime* (groups or coalitions controlling the highest authoritative policymaking bodies of government) and *political oppositions* (groups challenging the regime's power and/or policies). Highly abstract concepts such as these may maximize cross-national applicability but, in doing so, risk becoming so airy that they float far above any meaningful empirical referents. Not so with regimes and oppositions; for Hagan meticulously reviews scores of (non–U.S.) case studies to demonstrate (1) that political oppositions can be found across a broad spectrum of institutional structures from parliamentary democracies to various Third World systems and (2) that such oppositions do indeed matter in the conduct of foreign policy.

Sorting out just how oppositions matter is the central theoretical task of the book. Hagan constructs a conceptual framework around his two key concepts to accommodate what he regards as the two most significant sources of domestic political opposition. The first of these encompasses political divisions within the regime itself, arising from the ruling group's cohesiveness, its leadership structure, or its polarization over issues. Such internal divisions determine the extent of regime fragmentation. The remaining sources of political opposition are located outside the regime—from defecting members of the ruling party, from opposing political groups or parties, or from military or regionally based actors. Hagan theorizes that regime fragmentation and political opposition become relevant to foreign policy by sensitizing leaders to the twin political imperatives of coalition policymaking and retaining political power. Foreign policymaking is thus transformed into a two-level game whereby leaders must balance foreign policy goals with domestic imperatives by adopting a political strategy of bargaining and controversy avoidance or, alternatively, policy and regime legitimation. Hagan also recognizes that the stakes in this game are not uniform across political systems. Returning to the traditional second-image concern with political structure, he introduces a third, mediating element into his framework consisting of accountability, institutionalization, and (less traditionally) regime vulnerability.

From these basic building blocks, it becomes possible to draw out certain hypothesized effects on the broad contours of foreign policy behavior. For example, Hagan reasons that when the constraining effects of fragmentation or oppositions engender bargaining or controversy avoidance, the outcome is likely to involve diminished levels of commitment and assertiveness. Alternatively, politically constrained regimes adopting a strategy of policy or regime legitimation are more apt to react with amplified commitment and assertiveness. Although the logic of each hypothesis is unassailable, there is something rather unsettling about being offered opposite predictions from identical initial conditions. Hagan avoids the approach of treating the all-important political strategies as alternative and empirically adjudicable theories about how regimes cope with political constraints and, instead, views them as simply alternative choices in a menu of coping responses. Although Hagan's admittedly exploratory account moves well beyond simple second-image preoccupations with political structure, the choice of political strategy is so central to his story that we are left a bit unfulfilled by the lack of theorizing on this crucial point.

The systematic, cross-national applicability of this framework is demonstrated by Hagan's painstaking effort to encode the oppositions faced by 94 political regimes across a sample of 38 nations during the 1960s. Regime divisions are operationally demarcated by a basic change in regime fragmentation, that is, by a change in the ruling group or its structure, its leadership authority, or its issue polarization. Perhaps a familiar example will clarify this demarcation criterion. Hagan identifies three U.S. regimes from 1959 through 1968: Eisenhower, Kennedy–Johnson 1, and Kennedy–Johnson 2. The transition from Eisenhower to Kennedy is a fairly obvious regime change; but the transition from Kennedy to Johnson is not considered a change in regime, since the ruling group remained virtually intact. The second regime change occurs in June 1966 and reflects the increasing issue polarization over Vietnam in the Johnson administration. Drawing from literally hundreds of published sources, Hagan classifies each of the 94 regimes on a host of specific dimensions relating to levels of fragmentation (e.g., politically predominate leader is present or not), oppositions (e.g., ruling party is cohesive or highly factionalized), and contextual political conditions (e.g., system is highly institutionalized or not). This brief and highly simplified sketch of the coding process belies the enormous personal effort so obviously underlying the final set of regime classifications. Although to the generalist's eye Hagan's decisions appear perfectly reasonable, one is left to wonder about the reliability of these judgments in the hands of another coder.

The penultimate chapter in the book analyzes the regime classifications against a series of foreign policy behavior measures acquired from the CREON events collection. Unfortunately, the bivariate correlations on which Hagan relies almost exclusively proliferate so rapidly across his multiple indicators and subsamples of regimes that we are very nearly overwhelmed by tables of coefficients. And while Hagan's interpretations of these results are soundly reasoned and entirely plausible, there is nonetheless a touch of ad hoc rationalization about them. In fairness it should be said that despite the specific hypotheses around which the analyses are organized, this effort is really much more an exploratory descriptive enterprise than an explanatory or hypothesis-testing one.

In summary, the real value of Hagan's book is the dimension of depth it adds to the traditional second-image focus on manifest political structures. The book succeeds in delivering its central message that foreign policymaking is inextricably linked to domestic politics, largely on the strength of Hagan's extraordinary mastery of a widely scattered case study literature on foreign policy and the innovative conceptual framework that is subsequently synthesized from this literature. Equally impressive is the effort to transform key elements of this framework into a set of operationally defined indicators that are applicable to the full range of governing arrangements found in the contemporary nation-state system. Although the book is not without certain weaknesses, in the end, it does all that we should ever expect: it helps us to think about politics more systematically.

University of Arizona WILLIAM J. DIXON

Problems and Process: International Law and How We Use It. By Rosalyn Higgins. New York: Oxford University Press, 1994. 274p. $35.00.

The English jurist, Rosalyn Higgins, long widely known and respected, has, with this book, put herself in the front rank of international law scholars of the late twentieth century. *Problems and Process* is a remarkably sustained effort, covering the most crucial and controversial international law issues of the day, while at the same time articulating, with clarity, erudition, and conceptual elegance, an important jurisprudential position that is likely to exert influence on both sides of the Atlantic. Indeed, it is a peculiar virtue of this book, reflecting Higgins's preoccupation over the decades, to reconcile the best of the British approach to international law, with its heavy emphasis on the technical depiction of rules and doctrine, with the American emphasis on policy and context.

As Higgins explains in her preface, *Problems and Process* is a revised version of a series of 15 lectures delivered in the form of a prestigious *cours general* at the Hague Academy of International Law. Although it has a certain engaging informality of style, the quality of the scholarship and the texture of the reasoning have resulted in a work that needs to be read carefully and reflected upon and could not, at least in its present form, be very easily absorbed by oral transmission.

Higgins strongly believes that international law can contribute to human betterment in a variety of ways, especially if it is appropriately developed and applied. She does not believe that international law offers governments and international institutions clear guidelines when confronted by difficult policy choices but, rather, that it can help those in authority make better choices, identifying alternatives. It offers leaders and advisors assistance in balancing past expectations and present policy goals to achieve a course of action that is more likely to produce favorable results. Implicit in her outlook is an acceptance of the Western liberal tradition of respect for rights and promotion of democracy as the proper normative orientation. Using the traditional craft of a skilled lawyer, Higgins fully analyzes past treatments of issues by courts and scholars as the foundation for reaching her own conclusions, acknowledging contested zones of discussion (on, say, humanitarian intervention) and putting forth reasons for preferring a given understanding of law over alternative readings.

Another attractive feature of *Problems and Process*, especially for political scientists, is its attentiveness to the relevance of power. In many respects, both realists and liberals will find her overall treatment of international issues congenial. In Higgins's view, international law is shaped by what those in power decide, and it is primarily to help them decide more intelligently and constructively that international law serves human well-being. She is not stuck solely in the domain of power, being alive to the significance of human rights and the connections between claims of self-determination and the denial of democratic governance. In this respect, although it is not argued as such in the book, Higgins is an adherent of the currently popular view that the spread of Western liberal democracy to additional countries is the most practical and effective way to promote world peace.

As I have indicated, taken within its declared framework, *Problems and Process* succeeds brilliantly. It is a book that will be widely used, frequently cited, and likely to remain of interest for decades. It may well influence scholars in many parts of the world by its convincing synthesis of secular humanism and political realism. It also discusses some of the most important topics of pedagogy and substance in international law, including legal sources, jurisdictional matters, self-determination, the role of the United Nations, the work of domestic courts, and the use of force by individual states and by the United Nations. I regard it as the best available, most sophisticated, and successful introduction to the general field of international law. As such, it is a fine achievement.

In her preface, Higgins disavows any intention to be comprehensive with respect to the whole of international law. She sets out to explain her viewpoint as persuasively as possible, using theoretical and substantive issues to develop her central argument but also to illuminate some critical questions.

Leaving aside differences in interpretation on matters of substance and jurisprudence, I find Higgins's view of international law, despite my praise, to be not entirely satisfactory. There is, despite her good intentions about values and policy, a formalism about the presentation that discourages a reader from perceiving structures of privilege and domination that are characteristic of the way international law operates. In this respect, her presentation of international law is top-down, avoiding altogether any discussion of the relevance of unevenness among states and peoples with respect to wealth and military power. The tone is one of critical innocence that never ponders the degree to which intervention runs only north to south or that the projection of power has consistently served through history the purposes of the strong and rich at the expense of the weak and impoverished.

In conclusion, Rosalyn Higgins's book provides the best overview of the current role of international law. It is a balanced and reliable accounting of the achievements and limitations of international law, as well as a helpful guide to the way it works in relation to some of the main problems of international life. My only caveat is its implicit conservative stance, never questioning the extent to which law has served as an instrument of the strong against the weak throughout the history of international law. Transforming international law into an instrument for the weak to address issues of economic and social injustice is, unfortunately, not on Higgins's agenda.

Princeton University RICHARD FALK

The Benefits of Famine: A Political Economy of Famine and Relief in Southwestern Sudan, 1983–1989. By David Keen. Princeton: Princeton University Press, 1994. 289p. $47.00.

The study of the causes of famines and the international community's response to them is of great contemporary significance. David Keen has made an impressive and useful contribution to the literature on famines, addressing, in particular, the functions and benefits of famine and the role of war in producing and aggravating famines. Although this study indicates that it is contrary to Amartya Sen's works on famine, it is best seen as an important supplement to Sen, extending the study of famine to the most likely future candidates—powerless ethnic groups in conflicted societies. Keen overreaches in trying to use the Sudan as an exception to Sen's claim that there is a close link between democracies and famine prevention. As is well documented by the author, Sudan's 1986 election excluded over half of the southern constituencies, was a rare interruption in a long line of authoritarian governments, did not lead to a free press, and did not establish the Sudan as a democracy.

Keen's central argument is that a famine may have powerful beneficiaries within the affected country, among ethnic groups, merchants, and political elites at both the national and local level. Famine and the blocking of relief can serve important political and economic functions for a number of groups: as a weapon of internal warfare, attacking the food supply of rebels, displacing their supporters and potential recruits, and destroying the rebel economy; a producer of exploitative profits for grain and livestock traders; a source of loot and pillage for raiding private militias; and fees, diversions of relief goods, and other corrupt profits for some military and civilian leaders.

Using an extensive case study of famine among the Dinka in southern Sudan from 1983 to 1989 (providing updates into 1993), Keen displays the famine as a process involving the forced transfer of assets from a powerless victim group to various beneficiaries. Keen emphasizes that famine involves more than just the numerous starvation deaths seen at the culmination of a disaster. Earlier stages of population displacement, loss of assets and impoverishment, political powerlessness, disruption of a way of life, and chronic malnourishment are critical to understanding the famine process. The processes may be actively promoted by the beneficiaries of famine. He notes that the victims, the Dinka, initially were not impoverished. Indeed, it was their assets—cattle, grazing land, labor, and newly discovered oil reserves—that exposed them to exploitation while their political powerlessness offered them no protection.

Keen devotes two chapter to examining the "inadequacy of relief," particularly the silence and shortcomings of international donors and the United Nations in responding to the Sudanese and other famines in the context of internal conflict. The international community's efforts at famine relief during civil war suffer from the strictures of sovereignty and noninterference. Relief only goes to conveniently accessible areas. Internally displaced persons in rebel-held territory are deemed inaccessible, by means of government (one of the parties to the conflict) institutions and personnel. Because such international relief favors the government, rebel attacks on "humanitarian" aid should not be surprising.

Conventional relief practice gets a late start on famines because it focuses on the final stage of widespread deaths, rather than intervening earlier in the famine process. Food aid alone is unlikely to work in the context of war and the active creation of famine. Conflict exacerbates the normal power inequalities requiring greater attention to the needs of those who are powerless. In particular, relief assistance channeled through the hands of those who promote and benefit from a famine is unlikely to reach the victims unless the international donors actively monitor their assistance and press for its delivery. In the Sudan, hundreds of thousands of southerners died while donors blindly accepted government assurances it was doing everything possible to assist them.

Keen offers several policy suggestions for the future, particularly "exerting diplomatic and economic pressures on governments and other influential groups that are promoting famine" (p. 234). However, famine continues today in the Sudan despite its denunciation as an abuser of human rights and a sponsor of terrorism. When famine has beneficiaries, its promotion will not be easily deterred.

Although Keen notes the need to get relief aid into rebel-held areas he offers few suggestions on how this might be done. Working with rebel groups and their relief agencies, providing relief across borders, rather than through government channels, ignoring or sidestepping sovereignty in order to reach millions of internally displaced persons is one of the great contemporary challenges to donor governments and the United Nations. There is a need to develop standards, guidelines, and criteria for working with nongovernment forces controlling an area in order not only to deliver assistance to the hungry but to encourage and pressure these forces (which, after all, are claimants to international status) to respect recognized international agreements, human rights, and humanitarian law and to permit the monitoring of aid delivery.

This volume is a tough read. Keen's narrative style confuses as often as it enlightens; and valuable analytical highlights are buried in detail about dates, tonnage, and shipments. The chronology of events is often skewed: the 1972 Addis Ababa agreement ending the first civil war is primarily mentioned and described in back references after it is violated. The many ethnic groups (particularly the Dinka, Baggara, and Messiriya) are not introduced, and their numerous subdivisions are not clarified. Keen notes that the Mahdi called his supporters the Ansar and that one hundred years later the Ansar had five to eight million adherents, yet the structure of the movement or sect or party is not explained. Most confusing is a brief first mention of a party (DUP) or an agency (CART) followed by use of its acronym after a gap of dozens of pages. There are missing references, and the index has *United States* before *United Nations*.

Keen's book is a very useful addition to the famine literature. In an international arena filled with weak states, fragile peace, and warlords, Keen adds important layers to out understanding of the functions and benefits of creating famines and, hopefully, will improve our response to the processes of famine.

Michigan State University BARRY N. STEIN

The Vulnerability of Empire. By Charles A. Kupchan. Cornell: Cornell University Press, 1994. 527p. $35.00.

Why do states sometimes adopt prudent policies in response to power shifts in the international system, while at other times they engage in imprudent and, ultimately, self-defeating behavior? More specifically, why is it that states with far-flung empires, when faced with unfavorable power shifts, are unable to protect the core and allow themselves to be distracted by the peripheries? These are the key questions posed by *Vulnerability of Empire.* Based on case studies of the international behavior of Britain, France, Japan, Germany, and the United States, Charles Kupchan argues that the answer has much to do with "perceptions of vulnerability." When policymakers perceive their state's strategic vulnerability to be low (i.e., when they expect to prevail against adversaries should war occur), they tend to adapt well. Such were the cases when France in 1870–1904 and 1904–14, Britain in 1904–14, Germany in 1870–97, Japan in 1931–37, and the United States in 1945–49. As rising powers, some of these states could exploit changes in the international system to enhance their own power without provoking serious opposing coalitions. Even declining powers, such as Britain and France before World War I, were also able to adopt prudent policies (balancing the rise of German power) as long as they perceived their strategic vulnerability to be low.

The pathology occurs when perceptions of vulnerability are high, that is when policymakers feel that their own resources, combined with those of their allies, cannot cope with emerging threats and that they might actually be defeated in the event of war. According to Kupchan, Britain is 1932–39, France in 1932–39 and 1949–54, Japan in 1937–41, Germany in 1897–1914, and the United States in 1949–50 found themselves in such a position. High vulnerability caused Germany, Japan, and the United States, as rising powers, to pursue indiscriminate expansion in the core as well as in the periphery. Declining powers like Britain and France in the 1930s became "overly cooperative" in the core and indiscriminately defensive in the periphery, as exemplified by their appeasing Germany in their backyard while channeling resources to defend their far-flung colonies. All these policies pursued under high vulnerability were, in Kupchan's estimation, extremist and self-defeating.

The theoretical core of Kupchan's book lies in his specification of how perceptions of vulnerability result in self-defeating behavior. High vulnerability "leads to strategic beliefs that induce elites to adopt extremist policies" (p. 14). These policies (e.g., appeasement, overextension) require their proponents to drum up public and bureaucratic/military support, and they do so by invoking "specific images and conceptions of empire" (p. 15). In so doing, a new *strategic culture* supportive of these policies emerges. (Kupchan's use of the term is controversial. Nitpickers may substitute their preferred term, say, *strategic theory*, without affecting the argument.) By the time the elites realize that their policies are indeed extremist and counterproductive, they are unable to shift course, because the strategic culture they created now severely constrains their room of maneuvre. The strategic Frankenstein strangles its master and leads the nation to ruin.

Vulnerability of Empire is an important contribution to military security and foreign policy studies. Kupchan does an admirable job of integrating systemic, domestic, and cognitive variables to arrive at an explanation of his own. His specification of the ideas-domestic nexus or how ideas/images of empire interact with the domestic policy (hereafter, *domestic logic*) to engender self-defeating behavior is especially interesting. Kupchan posits a different and more complex causal model than that provided by Jack Snyder's *Myths of Empire* (1991). Kupchan specifies the conditions (high or low vulnerability) under which different strategic beliefs emerge, and he finds less evidence of logrolling and more evidence of elites colluding to define the strategic debate in order to get skeptics on board. Kupchan's (internationally inspired) domestic logic seems convincing for two reasons: (1) he shows that domestic pathologies did not feature prominently under conditions of low vulnerability but became prominent only under conditions of high vulnerability. There is thus is variation in his dependent variable. Second, his case studies, half of which are based on archival research, are well done and seem to fit the postulated behaviors.

A problem with the book is conceptual overstretch or redundancy. The use of term *strategic culture* has been alluded to earlier. The theoretical payoff of introducing *perceptions of vulnerability* is also questionable. Kupchan wants to unpack the dynamics of grand strategy decision making, a task neglected by structural theorists who view the correlation between international structure and state behavior as sufficient indication that the former causes the latter. The works of Robert Jervis, Deborah Larson, and Stephen Walt have shown, however, that such neglect of the intervening "perception" variable is often logically and conceptually unsound. Since the 1980s, the most interesting works have turned to domestic politics, beliefs, and perceptions. Kupchan follows this tradition in dealing with all three. As mentioned, his specification of the domestic logic (and strategic images) advances our understanding of the sources of self-defeating policies. The concept of perceptions of vulnerability however, adds baggage without giving equivalent payoffs.

As Kupchan's case studies show, states that acted prudently perceived correctly that they were not (strategically) vulnerable. States that acted imprudently also correctly perceived that they were vulnerable (pp. 35–37). In both cases, then, states perceived their strategic vulnerability correctly: the fit between reality and actor response is good. According to the structuralists, in cases like these, where the link between "reality" and behavior is unproblematic, there is no need to deal with perceptions. Perceptions are much more illuminating when there is a lack of fit between environmental inducements and actor responses. In other words, the concept of perceptions carry little of the explanatory load and complicates Kupchan's argument unnecessarily (although it does not undermine its substance). Most of the explanatory weight falls on Kupchan's beliefs–images–domestic politics nexus. It is the theoretical elaboration of the latter and how they generate wise and unwise grand strategies that will earn the book a prominent place in the reading lists of security studies courses.

Nuffield College,
Oxford University
YUEN FOONG KHONG

Beyond the Cold War: New Dimensions in International Relations. Edited by Geir Lundestad and Odd Arne Westad. New York: Oxford University Press, 1993. 257p. $39.95.

When President George Bush launched his phrase about "the new world order," he threw out a challenge to political scientists around the world to help bewildered policymakers rethink the basis of international relations. Very few have done so, preferring instead to act as politicians and ridicule the absence of the concept of reality. Quite obviously, the world is emerging from a clear, if complex, system of world order, operating on the basis of known but alterable ranks, relations, roles, and expectations. That loose bipolar system has collapsed; and its successor is still inchoate, with a number of contenders vying for dominance. This book is one of the few attempts, to date, to explore this evolution.

It is only mildly successful, for frustrating reasons. Essentially, this is an unedited volume with some good chapters. Papers appear not to have been revised since their initial presentation in 1991 at the ninetieth anniversary symposium of the Norwegian Nobel Committee. The problem is not that they are outdated. It is that they are not cross-referenced, not edited as chapters, not held to common standards, and not integrated into a book. If some authors have done a disservice to the collective enterprise, the editors have also done a disservice to some of the authors who have made thought-provoking contributions.

The best contributions are by John Lewis Gaddis of Ohio University and Raymo Väyrynen, now of Notre Dame University. Gaddis analyzes the late system of world order as a deep conflict over values, rather than simply over power structures, held in place by the balance of terror. Out of this system, two contrary characteristics of a new order emerge—integration and fragmentation—exerting competing pressures that can force the system into different directions. At a time when functional integration creates larger interactional units particularly in the developed world, identity devolves to smaller units, particularly in the developing world (and its encroachments on areas formerly considered developed). Väyrynen continues this theme in analyzing the evolution of international conflict, showing the developed world's primary concern with the consequences of transnational integration while developing regions seek new institutions of identity, sovereignty, and legitimacy. The ensuing conflicts, as the world's structures and interests pull apart, lead to new forms of interaction, in which even "center" and "periphery" give too sharp a notion of structure.

Other chapters contribute to this theme in regard to specific regions or functions. Alexander George, of Stanford University, addressing the same theme, is understandably somewhat outmoded in his discussion of the Middle East but pertinent and insightful in examining the importance of the United Nations and nonproliferation in future conflicts. Michael Howard of Cambridge University gives a sound review of arms control and a serious analysis of the future dangers of continuing production beyond control in the Third World. Shaul Bakhash of George Mason University, provides an excellent and discouraging analysis of the prospects for democratization in the Middle East, where the process favors, above all, the fundamentalists. Susan George of the Transnational Institute writes an excellent exposé of debt as a strategy of exploitation of the Third World that expands the Väyrynen analysis.

Thereafter, quality becomes more uneven. A number of chapters are "exhibits," typical reflections of a regional style and point of view but without much analytical depth. J. F. Ade Ajayi and Ali Mazrui mix facts and perceptions within an African context; Jerzy Tomaszewski gives a stream-of-consciousness list of East and Central European conflicts; Jorge Castañeda offers a frustrated picture of Latin America in international relations, where he sees drugs replacing anticommunism as the sole element of U.S. interest; and Vladimir Shustov and Yuan Ming give narrowly Soviet and Chinese commentaries on Gaddis's analysis of the Cold War.

A few other contributions round out the collection. K. Subrahmanyam gives a factual review of nuclear proliferation, Toru Yano combines a good study of the spread of democratization with a convoluted portrayal of decolonization; Richard Falk attempts to combat neorealism with assertions in the place of analysis; and Willy Brandt and Gro Harlem Brundtland write op-ed pieces—Brandt's as the introduction (as it should be) and Brundtland's, curiously, as one of the substantive chapters.

The book is a lost opportunity, marked by a few strong contributions, some salient insights, and a few thematic leitmotivs that could have provided the rails for a powerful analysis if the other chapters had joined in the collective enterprise. The collection is worth reading for those messages; and even the weak chapters can serve as a foil for inquisitive speculation on better ways to handle the problems. Perhaps there were too many noteworthy figures invited to participate in the conference to allow for an effective integration of views, messages, and egos; and perhaps the editors never tried. It is too bad; for the challenge of understanding the upsurging world in all its chaotic diversity is of the magnitude of Alfred Nobel and the conference participants.

Johns Hopkins University I. William Zartman

Power, Trade, and War. By Edward D. Mansfield. Princeton: Princeton University Press, 1994. 278p. $35.00.

Edward Mansfield argues in this book that there is a curvilinear relationship between the concentration of power and war at the level of the international system, based on data for five-year periods between 1825 and 1964; that is, war is most likely at intermediate levels of power concentration, least likely at either high or low levels. He also argues that war is more (less) likely when there are relatively low (high) levels of international trade flows. The two variables—power concentration and trade—explain a high percentage of variance in the war data. Thus, Mansfield concludes that a combination of neorealist and political economy perspectives is needed to explain war at the level of the international system.

The path to these conclusions is lengthy and difficult, given the disparate approaches to measuring war; but Mansfield does an excellent job of explaining his steps along this tortuous route. An entire chapter (chapter 2) is devoted to describing and comparing was data collections by previous scholars. Five different definitions of war and nine data sets derived from them are discussed in this chapter. It is interesting and somewhat disheartening that the correlations between system war measures in the different data sets is low. According to the author: "Given the low correlation between these data sets, analysts should be hesitant to use them interchangeably. This is not to imply that any of [them] is 'wrong' or misleading. Each is useful contingent on the objectives of the particular analysis" (p. 43).

One of the major subsidiary arguments of the book is that scholars have paid too much attention to "polarity"—the number of poles (sometimes equated with the number of great powers) in the system. Mansfield agrees with these authors that the distribution of power is a key potential causal variable but disagrees with their contention that polarity is the best way to measure that distribution. He argues, instead, for using a measure of concentration that takes into account both the number of great powers and the relative distribution of power across them. This argument is made quite persuasively.

Mansfield also suggests that other scholars have erred in testing only monotonic relationships between the distribution of power and war, demonstrating that a curvilinear model explains more variance. Again, I found this demonstration convincing.

Finally, Mansfield shows that multivariate models combining economic variables (trade levels in particular) with political ones (the concentration of power) explain a higher percentage of variance in systemic war levels then models that do not. This suggests to him that "interdisciplinary research between political scientists and economists needs to be conducted, and is likely to foster a fuller understanding of the relationships among power, trade, and war" (p. 253). Again, the argument was quite convincing.

There is only a short discussion in the book of its implications for current policy. Mansfield implies that the breakup of the Soviet empire "bodes poorly for the avoidance of war in Europe" but that "continued expansion of international trade offers an avenue for improving political relations while, at the same time, increasing global welfare" (pp. 252–53).

What is missing here, unfortunately, is a careful discussion of how far one can generalize or extrapolate from the type of systemic data used in the various data analyses. For example, Mansfield mentions briefly that there are reasons to believe that the introduction of nuclear weapons may have changed the relationship between the distribution of power and war but does not go on to explain why he fails to take the argument seriously.

Still, Mansfield should be praised for the care and skill he demonstrates in dealing with a wide variety of theories, data sets, and statistical methods. The prose is a bit tortuous and therefore not suited to use in introductory courses; but for graduate students about to undertake their own empirical quests, it would be hard to find a better exemplar.

Indiana University Jeffrey A. Hart

Mercantile States and the World Oil Cartel, 1900–1939. By Gregory P. Nowell. Ithaca: Cornell University Press, 1994. 326p. $45.00.

At its core, the field of international political economy remains rightfully concerned with relationships between firms and states in the global economy. The modes of inquiry shift quite frequently in the discipline, as do the subjects, moving from firm back to state, from local to international markets, and from quantitative to qualitative analyses. The basic questions, though, remain largely the same: How do firms affect the behavior of states? How do states shape and constrain the profit-making potential of firms? And how do the two types of actors interact in, and ultimately create, the world economic system?

In *Mercantile States and the World Oil Cartel*, Gregory Nowell brings a clear and decisive voice to the ongoing dialogue of the field. Unlike many recent works that confine themselves to minor corollaries of existing theories or small areas of historical debate, Nowell's work is remarkably ambitious, tackling simultaneously realism, mercantilism, etatism, and the great oil cartel of the early twentieth century. His theoretical claims are bold and, potentially, disruptive. Realism, he argues, is simplistic and flawed. So are its mercantilist and liberal-regime variants. In their stead, Nowell offers a new approach, *transnational structuring,* that moves away from the nation-state and takes, instead, the firm as its unit of analysis.

Specifically, Nowell suggests that multinational corporations have played a far greater role in world affairs than realism awards them. What looks like state-initiated action, he argues, is often a state response to well-concealed corporate maneuvering. The "national interest," particularly with regard to economic intervention or regulation, is often but the profit-making interest of the state's most powerful business entities. Thus, what looks like "reality" to the realists is only a "useful illusion" to the firms that concoct and drive these ostensibly realist policies (p. 32).

In making these claims, Nowell borrows clearly from existing theories of interest group pluralism and rent seeking. But he goes much further, arguing that the process by which firms shape state policy occurs largely at the international level. To understand national regulation, he contends, we must look at the international behavior of firms—a process he defines as transnational structuring. Somewhat more formally, he describes transnational structuring as "the means by which the normal political and economic lobbying activities of capitalism can lead to the creation of new state institutions" (p. 5). Rather than responding to state policy, corporations thus shape it, helping to define along the way the very concept of national interest that realists hold so dear. More dramatically, they even begin to encroach on the power of governments. Or as Nowell writes in an unusually Marxist moment, "Because of the smoke screen of national interest rhetoric, world capitalist markets can be rationalized under our noses" (p. 8).

To support these powerful claims, Nowell examines French energy policy between 1900 and 1939. He chooses France because the predominant view is of a "strong" state and because, he asserts, "what happened was ordinary" (p. 2). By showing that France was not, in fact, strong in the face of corporate pressures, Nowell seeks to demonstrate that "corporations structured the world of states, not as nationally based lobbies striving for occasional modifications to purely national laws and policies but as agents for the control of international economic development and the dispersion of international technological abilities" (pp. 2–3).

It is a provocative assertion and one clearly demanding considerably more study. But the links between evidence and proof and between explanation and theory also suffer some considerable flaws. To begin, France's response to the world hydrocarbon cartel is a curious event from which to launch the ambitious theories that Nowell proposes. The French response may have been "ordinary," but the oil industry of 1900–1939 surely was not. In fact, it was characterized by one of the grandest and most successful cartels of all time. So generalizing from the behavior and influence of firms in this industry in this era is somewhat problematic. Certainly, there are contemporary industries where one might expect high levels of corporate influence (e.g., semiconductors, aerospace); but oil in the early twentieth century still seems far more the exception that the norm. During this time, the oil industry boasted nearly all the characteristics liable to facilitate both an international cartel and a considerable level of political influence: oil was a strategic commodity with high barriers to entry, a handful of key players, tremendous scale economies, and only a few fringe or independent producers. Few other industries bear such a propitious set of factors.

More importantly, for transnational structuring to warrant attention (let alone, replace realism), it must present evidence, one would assume, of conscious intent; that is, somewhere, we need to see the firm or the industry deciding to restructure the global market and then realigning its local activities accordingly. Empirically, this implies doing research within the firm, rather than merely imputing strategies from either behavior or outcomes. Of course, as Nowell argues, this sort of research is very difficult. But *Mercantile States* relies, paradoxically, almost exclusively on government sources and never presents convincing evidence of how the great oil firms defined their own objectives and strategies. Thus, not only does it lack the fascinating stories of the companies and the men who ran them, but it also makes the very same theoretical leap for which the author condemns realism. Nowell employs an admirable range of primary source material, but he still cannot demonstrate how the companies viewed the world or the markets they purportedly strove to control.

Finally, it is ironic that despite the influence the oil companies clearly had in shaping state policy, in the end the states triumphed. United States antitrust law severely limited Standard Oil's control; World War II shattered the cozy relationships of the Achnacarry agreement; and led by tiny Libya, the oil-producing countries ultimately broke the last vestiges of the great oil cartel. None of these events necessarily undermines Nowell's concept of transnational structuring; but they do need to be considered if one wants to piece together the highly complex story of how firms and states interact in the global energy market, let alone the international system.

In his final chapter, Nowell sketches a highly convincing metaphor of a three-way board game. To the realists' two-player chess, he adds two simultaneous games: a checkers match between competing firms and a *go* match between firms in competing industries. Each set plays by its own rules, but the patterns and maneuverings of each game critically shape the environment in which the others occur. It is a compelling picture, one that intuitively seems correct in describing the complex interactions that together determine the international system. Insofar as it focuses predominantly on the game of the oil firms, *Mercantile States* encounters the same limitations of many of the works it criticizes. But insofar as it pushes toward capturing the full complexity of the board, it is a powerful piece of scholarship and a provocative guide for future research.

Harvard Business School DEBORA L. SPAR

Reciprocity, United States Trade Policy, and the GATT Regime. By Carolyn Rhodes. Ithaca: Cornell University Press, 1993. 249p. $32.50.

Two distinct strains of thought govern the study of the political economy of international trade. They can be summarized (admittedly in a very stylized and stereotypical fashion) as follows. Consider two states, Home and Foreign, each of which can choose to follow a strategy of trade liberalization (opening domestic markets to imports) or a strategy of trade protection (maintaining high trade barriers). There are four possible outcomes: (1) both states follow a strategy of trade liberalization, (2) both states maintain high trade barriers, (3) Foreign follows a strategy of trade protection, while Home opens its markets to imports; and (4) vice-versa. *Classical free-traders* argue that both states are best off under universal free trade and worst off under universal protectionism. More importantly, if, for some reason, Foreign maintains high trade barriers, then Home is better off *unilaterally lowering its trade barriers*—and vice-versa. (Lower trade barriers are *not* considered a concession to importers because they increase the aggregate welfare of the importing society.) It follows that each state has a dominant strategy of engaging in free trade; that is, each state should lower its trade barriers regardless of what the other state does. The model thus predicts that we should observe universal free trade.

Of course, in its extreme form, this theory is empirically refuted by the pervasive presence of protectionism in international trade relations. The free-traders' response to this empirical observation is to assign the blame to "bad" domestic politics. Even though states would be better off *in the aggregate* if they lowered their trade barriers, the politics of trade protection are driven by the *intrastate distributional effects of opening domestic markets to international trade*: import-competing producers lose while consumers gain, and trade barriers are maintained because governments are beholden to producer interests. This setting can be conceptualized as a prisoner's dilemma: the producers of any one import-competing good prefer to have their profits, wages, and jobs protected by trade barriers; but in their role as consumers, they would be better off if protectionist barriers on all goods were removed.

Free-traders see the General Agreement on Tariffs and Trade (GATT) as a solution to this political problem. GATT norms, rules and procedures are designed to change the payoffs of the domestic prisoner's dilemma. In the model developed above, if the GATT successfully links the lowering of trade barriers across import-competing goods, then affected interests in Home and Foreign will be forced to choose between low or high trade barriers on all goods, with the result that they will support free trade.

In the last decade, strategic trade theory has emerged as a new theoretical rationale for protectionism, along with the policy implications that a state may strategically use trade barriers to improve its terms of trade or gain other advantages at the expense of the states. According to the *New strategic traders*, the game of international trade has the characteristics of a prisoner's dilemma. As before, Home and Foreign are best off under universal free trade and worst off under universal protectionism. However, if Home cooperates by opening its markets to Foreign's imports, then Foreign has an incentive to defect by maintaining its trade barriers—and vice-versa. The equilibrium of this international prisoner's dilemma involves both states following a strategy of trade protection.

The driving force behind the maintenance of high trade barriers now lies with the *distributional effects of trade protection across states*, leading to states' (ultimately futile) attempts to gain a competitive advantage at each others' expense. Strategic traders see the United States' unilateral efforts to open foreign markets via retaliatory threats—embodied most prominently in Section 301 of U.S. trade law—as the solution to this international prisoner's dilemma. In the model developed above, if Home links the lowering of its trade barriers to progress in opening Foreign's markets for Home's goods, then Foreign will be forced to choose between universal free trade and universal protectionism, resulting in trade liberalization. (The retaliatory threat to close Home's market is not illiberal in effect, because it is not realized in equilibrium: as long as the threat is credible, Foreign will cave in.)

Free and strategic traders thus disagree on (1) the importance of domestic versus international sources of trade protection and (2) the validity of multilateral (the GATT) versus unilateral (U.S. trade laws) trade liberalization strategies. Free-traders argue that the unilateral approaches à la Section 301 lead to a net increase in trade protection: if the threat fails and Foreign does not lower its trade barriers, then Home's trade barriers increase. Moreover, U.S. market opening policies are more than likely to fail because they are not motivated by the desire to provide enforcement to a liberal international trading regime. Instead, the U.S. government is captured by domestic producers who have a stake in the trade protection they are afforded if the retaliatory threat is realized. Last but not least, aggressive unilateralism undermines the effectiveness of multilateral solutions à la the GATT. (Indeed, Section 301 measures have often been classified as "GATT-illegal.")

Strategic traders, on the other hand, discount the role of domestic political distortions that could lead to the protectionist abuse of unilateral trade policy instruments in the United States. To the extent that they explicitly make an issue of domestic politics at all, it is to argue that the "bad" domestic politics of *other* states (Japan!) are creating the problem. Strategic traders are optimistic about the effectiveness of retaliatory threats in opening foreign markets, especially when compared with the GATT. In their view, the enforcement mechanisms of the GATT regime are too weak to prevent some states from free-riding on the liberal trade policies of other states. (Indeed, the GATT has repeatedly been pronounced "dead.")

Carolyn Rhodes's work, *Reciprocity, United States Trade Policy, and the GATT Regime*, contributes to this debate by synthesizing these two views about the political economy of international trade. She points out the ambiguous role of reciprocity in furthering trade liberalization or trade protection, analyzing the GATT regime and U.S. trade policy in an integrated fashion.

Rhodes's historical analysis of U.S. trade policy distinguishes three periods: the highly protectionist period preceding the 1934 Reciprocal Trade Agreements Act; subsequent decades of trade liberalization, in which the GATT was established and the United States played the role of a benevolent hegemon providing the "public good" of a liberal international trading regime; and finally, the period beginning in the 1970s, when the United States partially turned away from the GATT in favor of bi- and unilateral trade strategies. Three case studies deal with U.S. attempts to open foreign markets in the latter period, covering the interaction between the United States and its three major trading partners (Japan, Canada and the European Community) over traded goods produced in three sectors (steel, automobiles, and wheat flour).

Rhodes's historical analysis and case studies are full of intriguing detail and insightful discussion, and her book is a fascinating read. Her empirical analysis successfully makes the case for her central hypothesis regarding the ambiguous nature of reciprocity in furthering trade liberalization or protectionism. Reciprocity played a malevolent role in the protectionist environment of the pre-1934 period, a benevolent role under the liberal international trading regime that emerged subsequently, and an ambivalent role in recent market opening efforts on the part of the United States.

However, I have to admit that I was disappointed in a number of respects. Rhodes's historical analysis fails to provide theoretical underpinnings for, and disentangle, the relative contribution of changes in ideas about trade policy, the relative power of political constituencies and of parties, political institutions or regimes, and the international environment. Similarly, her case study analysis lacks a clear analytical framework of the kind sketched above. As a consequence, she amasses empirical details that do not appear to "prove" anything other than that lots of things can happen. Finally and amazingly, Rhodes overlooks recent developments in international trade theory (specifically, strategic trade theory) that provide an economic underpinning and rationale for current U.S. trade policy. (Paul Krugman, quite possibly the premier international trade economist in the United States, articulately describes the new developments in international trade theory in a number of articles and books that are accessible to the educated noneconomist, e.g., *The Age of Diminished Expectations* [1990] and *Peddling Prosperity* [1994].)

Let me illustrate my critique of Rhodes's work with two examples. To explain the sea change in the direction of U.S. trade policy—from trade protection to trade liberalization—in the early 1930s, Rhodes advocates the well-known "Congress sees the light" hypothesis: upon experiencing the disastrous consequences of the protectionist Smoot–Hawley Act of 1930, Congress began to understand that it would have to lower U.S. trade barriers in exchange for U.S. goods and services to be welcome in foreign markets. Alternatively, the change in U.S. trade policy could be due to the fact that pro-free-trade Democrats gained control of the presidency and the Congress in the early 1930s, replacing the protectionist Republicans of the time. The 1930s also saw a shift in power from Congress to the executive on many policy dimensions, including international trade. Assuming that the president is more sympathetic to free-trade ideas than is Congress (an assumption that needs to be motivated), the delegation of authority to the president, by itself, could have been driving factor underlying the shift in U.S. trade policy. It is also possible that Congress delegated power to the president with the understanding that it thereby committed itself to trade liberalization. But if Congress saw the light, why would it need to make such "institutional" commitments? Finally, there is the role of the international environment—World War II and the associated destruction of pretty much all of world industrial capacity except for that of the United States. Rhodes covers these possible causal factors in a descriptive fashion but fails to provide theoretical underpinnings, disentangle the relative contribution of various factors, and provide conclusive evidence that the explanatory power of the "Congress sees the light" hypothesis dominates that of alternative hypotheses.

The results of the case study analysis are similarly inconclusive. The U.S. strategy of aggressive unilateralism triggered counterpressures and retaliatory threats on the part of the Canadians and the Europeans, whereas the Japanese, in effect, caved in. As a consequence, the former were awarded preferential treatment compared to the latter. So what is special about Japan? The U.S. strategy was partially successful for steel and automobiles but led to a counterproductive stalemate over wheat flour. So what is special about agricultural products? The limitations of Rhodes's largely atheoretical case study approach are well illustrated by her "anything goes" summary for the automobile case: "My study demonstrates that dispute outcomes between highly asymmetrical trading partners are not necessarily predictable and that bargaining strategies and the character of market interdependencies affect outcomes" (p. 197). It is, of course, unfair to judge a whole book based on one unfortunate sentence, but the above conclusion summarizes in a compact way why the case study method has got a bad name. Is the case study data supposed to generate predictions, as opposed to theory? Do bargaining strategies drop from heaven in the same way that exogeneously given environmental conditions (such as market interdependencies) do, or are they a matter of choice?

In spite of its limitations, I believe Rhodes's work is an important empirical contribution to the *integrated* understanding of the GATT regime and U.S. market opening activities, which are usually analyzed independently: it is a must read for any serious scholar of the political economy of international trade. (And in all fairness, it is worthwhile noting that the weaknesses of Rhodes's book are not unique to her work: they beset much of the traditional international relations literature.) Rhodes's historical and case study analyses clearly demonstrate that neither of the two schools of thought summarized at the outset of this review have a lease on the Truth: the nature of reciprocal strategies in bringing about trade liberalization or protection is clearly more ambiguous than is suggested by the extreme views of either side. At the same time, the precise nature and relative importance of domestic and international prisoner's dilemmas appear to determine the degree to which reciprocal trade strategies will be trade-liberalizing or protectionist in effect. Yet the theoretical underpinnings of competing claims about the benevolence or malevolence of reciprocity in international trade are not fully developed, as a result of which the empirical research on these issues remains rather ad hoc in nature.

University of California, Los Angeles SUSANNE LOHMANN

Foreign Policy Restructuring: How Governments Respond to Global Change. Edited by Jerel A. Rosati, Joe D. Hagan, and Martin W. Sampson III. Columbia: University of South Carolina Press, 1994. 328p. 27.95 paper.

It is now becoming clear that the restructuring of the foreign policy of a state can lead to profound changes in world politics. The shifts in Soviet policy initiated by

Gorbachev ended a long-term and dangerous rivalry that looked like it would go on forever. In addition, his changes set off a train of effects that have catapulted us into a new historical era with new issues, actors, and even a new global political structure. If it were not for the restructuring of a major state's foreign policy, these transformations would probably not have come about. This volume looks theoretically at the general question of why states restructure their foreign policy.

As such it could not be more timely. The book focuses on what is meant by restructuring, how can it be explained, and what its implications are for the study of international relations. Such theoretical concerns are of paramount importance, and the editors and contributors have gone a long way toward setting out a research agenda that will provide important knowledge about foreign policy. In doing so, they have advanced the field of foreign policy analysis, which has been somewhat moribund of late because of the lack of a clear and measurable set of dependent variables.

It should come as no surprise that the most important contributions in the book are its conceptualizations of foreign policy restructuring and the various frameworks provided to describe and potentially explain the factors related to it. Of particular significance is Hagan's chapter, which develops typologies of various regime changes, changes in regime orientation, and potential regime constraints that can be used to ascertain the extent to which leadership and other domestic political changes may lead to foreign policy restructuring. Hagan postulates that leadership turnovers only lead to restructuring when the orientation of the regime, particularly its ideological orientation, has shifted significantly and when domestic constraints have weakened.

The book begins with an emphasis on why change is rare. The authors see foreign policy continuity as being a function of bureaucratic politics and domestic political interests that institutionalize these policies and of the constraints imposed by the global structure. Several of the chapters are devoted to identifying the stabilizers and institutionalizers of policy that provide overall continuity. Peter Schraeder sees bureaucratic incrementalism as an important factor in U.S. foreign policy toward Africa because of the low salience of this region to top leaders. In a nice quantitative analysis of U.S. policy behavior toward the USSR from 1948 to 1988, William Dixon and Stephen Gaarder find a great deal of continuity, with changes in presidencies having little effect. Thomas Volgy and John Schwarz provide a list of the major webs of constraint along with some important empirical findings that suggest that small democracies respond to external disturbances and large states to domestic pressures. In an interesting analysis, Steven Greffenius applies Kjell Goldmann's list of stabilizers (*Change and Stability in Foreign Policy*, 1988) to the Camp David accords to explain why it was so difficult for Israel to change its policy (because of the domestic political environment) and why the peace has generally been maintained. (He places great emphasis on the mutual acceptance of borders as a critical international stabilizer, a point consistent with some recent findings in peace research.) David Skidmore sees domestic political interests in domestically "weak" states like the United States as so powerful that they can prevent adjustment even to hegemonic decline. Clearly, there are many factors that prevent change, and these often work in tandem with each other. All this makes for a great deal of inertia even if there are leadership changes.

Nevertheless, imaginative leadership can be critical, as Sampson shows in his analysis of Libyan foreign policy, where he argues that both King Idris and Qadhdhafi were able to bring about significant economic gains for a small state by exploiting the "seams" in what appeared to be an all-powerful international structure.

The major implication of such analyses is that if change should occur, it will come about because of fundamental changes in leadership (like that reflected in the rise of Gorbachev) and/or fundamental and dramatic threats in the international system. Of course, the two factors may be interrelated. Park, Ko, and Kim provide some support for the former by showing that democratization in South Korea and Taiwan produced leadership changes that led to radical changes in foreign policy. A second implication is that *if* change should occur, it is likely to be abrupt and reflect a discontinuity, rather than a gradual change. Rosati takes this tack in his analysis of U.S. foreign policy, arguing that the history of recent policy shows that the U.S. follows a cycle where there is a lengthy period of stability followed by a period of transition, followed by a new stability.

What determines the balance between constraints and sources of change? Michael McGinnis, in his analysis of arms acquisition policies of regional powers, sees outcomes as a rational response by leaders to various constraints. Such a rational choice explanation does not always pinpoint the specific sources of fundamental change in foreign policy. Rosati states that the two major watershed events (and thus catalysts of change in recent U.S. foreign policy) were World War II and the Vietnam War. This suggests that learning, especially from traumatic war experiences, has a profound effect on foreign policy elites and probably existing domestic political alignments, as well. Schraeder, whose chapter is one of the best in the book, provides a more general model that can subsume this insight. He argues, albeit only for U.S. policy toward Africa, that when an issue is of low salience, bureaucratic preferences and general policy objectives set out long ago by the executive will guide policy until a major crisis emerges. This will then bring in the political leadership and provide some chance for change. If the crisis becomes extended, only then is it likely that the legislature and interest groups will affect a change in policy. Once a new policy becomes accepted the cycle repeats.

The editors recognize that they are still a long way from moving beyond a framework to an actual set of specific propositions that will explain foreign policy restructuring. They have made an interesting start, however. Although primarily a book for foreign policy theorists, most political scientists with even a passing interest in foreign policy will benefit from reading at least the last chapter.

Vanderbilt University JOHN A. VASQUEZ

Scientists and the State: Domestic Structures and the International Context. Edited by Etel Solingen. Ann Arbor: University of Michigan Press, 1994. 272p. $49.50.

Etel Solingen has undertaken, in this edited volume, a potentially useful and difficult, though ultimately flawed, task, namely, to relate what she calls the political economy of national scientific communities to the political and economic situation of the states in which they are embedded. Her underlying assumption is "that domestic structures and international context influence the nature and characteristics of national scientific communities and their relation to the state" (p. 23). Not a particularly startling idea but well worth probing if only to better understand and with solid evidence what is common among scientific communities in different countries, what elements prove to vary with the political environment, and how a highly interactive international political and economic system influence them. Along the way, such an analysis could provide systematic knowledge of increasingly important state-science relations in a variety of state settings.

Her approach is necessarily comparative, assembling studies intended to follow a roughly standard framework of the United States, France, Germany, Japan, China, the Soviet Union (through Gorbachev), India, Israel, and Brazil. Particular attention is given to the physics community in each country. The original idea may have been to follow a fairly tight design for all of the case studies, but reality has dictated a much looser framework "conceived as empirical plausibility probes . . . not designed to test a comprehensive and fully developed body of propositions" (p. 3).

Solingen conceives of four models that encompass a decreasing ratio of autonomy (of the scientific community) to accountability (to the state): *happy convergence, passive resistance, ritual confrontations,* and *deadly encounters*. The situation in each country can vary over time, or in portions of the community as political or ideological conditions change. By and large, most of the Western countries and Japan fit into the happy convergence category today, though during the time of Nazi Germany, passive resistance applied. Ritual confrontations were the norm in authoritarian developing countries such as Brazil in the 1970s, and deadly encounters, in periods of major stress such as the cultural revolution in China and Stalinism in the Soviet Union.

The ideas that illuminate the individual studies are of considerable interest; but in the end, practical and intellectual difficulties greatly impair the final product. Most important is the substantial unevenness in the chapters. Some provide excellent summaries of the situation in their respective countries, notably the chapters on the United States, France, China, the Soviet Union, and Israel. The chapter on Germany is good but gives little attention to the current scene; that on Brazil is much too brief and unanalytic; the India analysis loses its way, with its argument often unclear; and the chapter on Japan is so inadequate it should have been deleted from the volume.

Even the good summaries, however, do not necessarily adhere to the intended framework, so that comparative conclusions are hard to draw. In fact, in some cases, attempts to follow the framework seemed to impede the coherence of the presentations and make them difficult to follow. The Smith chapter on the United States, for example, is well done but could have appeared in any volume presenting a compendium of science-state relationships. Perhaps the incoherence of the chapter on Japan was also partly a result of trying too carefully to follow a common pattern of questions.

Solingen's introductory chapter reflects, in part, her own difficulties in encompassing the material that turned out to have only limited coherence in relation to her original intent. The selection of the four models seems somewhat unbalanced, since the first happy convergence model is very much broader than the others and helps neither to differentiate among, say, the United States, France, Germany, and Israel nor to discriminate among different segments of the community or of the state. The insights that the work could have produced are thereby hard to discern, or are unclear.

Finally, it must be noted that this book cries out for serious editing. Several of the chapters are hard to understand simply because of poor wording that any good editor should have caught. The chapter on Japan is the worst, but even Solingen's introduction is not as clearly presented as it could have been. Moreover, there are many examples of ambiguous antecedents, actual grammatical errors, misspellings in citations, and controversial assertions without citations provided. Much of the responsibility necessarily lies with Solingen, but the University of Michigan Press also was surprisingly derelict in releasing the volume for publication without either providing adequate editing help or insisting on necessary changes. The editing shortcomings are a pity, for they detract from the considerable knowledge and subtlety that Solingen demonstrates in her approach to the subject of state-science relations.

Massachusetts Institute of Technology EUGENE SKOLNIKOFF

The Cooperative Edge: The Internal Politics of International Cartels. By Debora L. Spar. Ithaca: Cornell University Press, 1994. 273p. $29.95.

Cooperation in international politics remains an elusive goal for nation-states. The absence of a central authority to govern states' activities poses one of the central challenges in their pursuit of collective objectives. Often these involve promoting peace and the spread of human rights, as well as increased economic benefits and decreased environmental degradation. In spite of the importance of these collective activities to international political stability, states often break commitments driven by pressures from narrow domestic political interests or a perception on the part of key policymakers that the interests of their state would be better served by pursuing policy objectives independently their potential collaborators.

Debora Spar tackles this question, Why cooperate?—Or more precisely, why not? In *The Cooperative Edge,* she develops a conceptual framework to explain why both private and public organizations occasionally succeed but for the most part fail to cooperate in situations in which cooperation would yield obvious, substantial benefits. The empirical analysis focuses on raw materials cartels to obtain insights into "why states cooperate" and "how they can build institutions to nourish and sustain their cooperation" (p. 1).

Spar uses market structure as a foundation upon which to build an argument about "the cooperative process and the behavior that facilitates it" (p. 5). Her analysis emphasizes the contribution of process, rather than structure, to the success of cooperative endeavors. This concern with process leads her to focus on the internal characteristics of the participants, both firms and states. The result is a fascinating and often colorful account of the personalities, strategies, and tactics in the diamond, uranium, gold, and silver markets that account for both the successes and failures in producers' abilities to stabilize their market through restricting supply and buying up surpluses for their long-term economic gain.

Using concepts from industrial organization and management theory, she devises a conceptual framework with which to analyze the behavior of both public and private actors within the individual markets. Spar begins with the observation that neither states nor firms can be understood as monolithic or as having uniform preferences. While the preference of state agencies will vary depending upon the extent to which they can act autonomously from interest groups or other political pressures, firms also face a complex array of stakeholders, some with countervailing interests. The internal structures of both firms and states, in turn, affect their abilities to formulate and implement strategic objectives effectively and thus become a key variable for explaining both the presence and absence of cooperative behavior.

For Spar, cooperation emerges out of four internal characteristics of principal participants (her term for the state and firm agents who have the principal responsibility for managing cooperative relationships within a cartel). She argues that these internal characteristics help to explain the emergence of cooperation in the cartel cases and may be generalized to instances in which other kinds of competitors (i.e., states) cooperate. The first involves the internal autonomy of the competitors and their abilities to make credible commitments. In the context of cartel management, this involves the ability of the principal participant to discipline internal factions and follow through on both costly promises and threats. Second, Spar suggests that cooperation in part depends upon the principal participant's perception of the shadow of the future. This may vary depending upon her expectations of length of tenure in office. Thus, the discount rate used in assessing the benefits from cooperation may differ from that of the "ideal" rational actor assured of an indefinite future with the organization. The gap between the personal and the rational discount rate may result in the perception that the initial costs of cooperation outweigh the long-term benefits as perceived by the principal participant. Third, for cooperation to succeed, principal participants must be rich enough and have the self-discipline to forgo current gains. Finally, since defection remains a constant possibility in cartels, the principal participants needs to have the resources to threaten potential defectors credibly and to punish actual defection effectively through flooding the market and driving a commodity's price below that offered by the defector.

In the empirical cases, Spar traces a range of cooperative and noncooperative behaviors in natural resource sectors all with industry structures conducive to cooperation to explain the underlying causes of the variation. In the diamond case, one of the most colorful, she carefully documents the mechanisms used by the cartel to manipulate supply. These include the "plain brown shoe boxes" in which DeBeers, the South African head of the diamond syndicate, used to distribute diamonds to buyers (called "sightholders"). She explains that "the buyers must either take the entire contents of their allotted parcel or turn all the stones back" and observes that "generally they take the parcel" (p. 55). To ensure that sightholders would not turn around and sell these diamonds at substantially higher prices, thereby creating an attractive alternative buyer to the syndicate for producers, DeBeers would strip noncooperative sightholders of their privileges. DeBeers also joined forces with the former Soviet Union to stabilize diamond prices. In often clandestine meetings, Soviet officials from the state agency responsible for exports of precious metals and jewelry (Russalmaz) would meet with DeBeers's executives to establish the price at which DeBeers would purchase the majority of Soviet diamond production. DeBeers saw this as a necessary strategy to keep the Soviets from flooding the diamond markets and driving prices down while the Soviets cooperated with their political adversary to increase their hard currency holdings.

Spar attributes the success of the diamond cartel to the centralized power of both the Soviet and South African states and the political autonomy of the individuals responsible for negotiating the ongoing cooperative agreements. This was matched by the "empire" built up by the Oppenheimer family in the DeBeers Company and the Anglo–American corporation. She describes both of these companies as run by a very tightly centralized top management team with the autonomy to make decisions that might violate the short-term interests of the shareholders in order to keep the cartel in tact. As a result commitments made by each of the parties to one another were credible. Not only were the participants willing to cooperate and reach compromises, but each had the authority "to implement these compromises at home" because of their autonomy and centralization (p. 219). They also had the capacity to punish those who broke commitments.

Some of the principal participants in the other cases do not have this kind of political autonomy. The government of Canada lost its nerve as a cartel participant in the uranium case because the secrecy demanded by the cartel posed a threat to its political culture. The U.S. silver lobby, in the silver case, forced the executive branch to renege on commitments the United States made to stabilize the silver market in the 1933 London Economic Conference. Both of these cases demonstrate the difficulty that democratic states have in maintaining the kinds of cooperative behaviors found in successful cartels. Spar's analysis suggests that democratic states often do not have the internal autonomy to impose compromises reached within cartels on domestic interest groups or the political support needed to punish cartel participants who stop cooperating.

Although the book identifies the political costs for cartel participants, the analysis does not really include a consideration of the political implications of the economic costs that successful cooperation within cartels imposes upon consumers. The primary objective of a cartel is to raise prices above those which would result from a competitive market. If these prices exceed the true scarcity value of the commodity, consumers suffer huge welfare losses as a result. Spar does point out that DeBeers's ability to control the price of diamonds and to promote the illusion that they were both scarce and very valuable dramatically inflated the cost of engagement rings. The Oppenheimer family may have been very shrewd negotiators skilled at compromise and intent upon reaching cooperative agreements even with one of their country's major political adversaries. But the goal of their cooperative behaviors was entirely monetary gain, and they were willing to act ruthlessly to preserve their financial control of the diamond market.

Can we learn about cooperation and what processes promote cooperation from these kinds of cases that can be generalized to other collective concerns of international politics? Spar argues that we can but that especially for democratic states, the political costs of cooperation may be too high. She concludes that the case studies portray cooperation as "a rather harsh game" and observes that "the very act of cooperation may require a political configuration that some [democratic] states may not abide" (pp. 243, 255). But not all international cooperation is based upon economic self-interest. Cooperation among states may find political support in democracies if it is based upon other, more broadly shared values. Spar provides a compelling account of the kinds of cooperative mechanisms necessary when participants' objectives involve narrowly defined economic gains. The analysis of cooperative processes in issue areas motivated by other values may offer alternative perspectives on how to reconcile democratic representation and political accountability with the ever increasing need for international political cooperation.

University of Minnesota STEFANIE ANN LENWAY

From Polaris to Trident: The Development of the U.S. Fleet Ballistic Missile Technology. By Graham Spinardi. New York: Cambridge University Press, 1994. 253p. $54.95.

Why do states build the weapons they do? This question is lies at the heart of any understanding of arms races, nuclear proliferation, or arms control. The answers given in the large literature on these subjects fall into three general categories. First, some scholars envision a kind of technological determinism: weapons technology advances out of control, pushed by its own "natural" momentum, and produces unnecessary or even counterproductive military modernization. Second, there is the "top-down" view of weapons acquisition given in most realist treatments of the issue: rational states must balance against rivals and therefore develop new weapons systems when they are the best option to counter external threats to national security. Third, there are "bottom-up" organizational and bureaucratic politics explanations that find the essence of acquisition not in perceptions of the national interest, but rather in the parochial interests of military organizations, weapons laboratories, or defense department officials.

In the past, scholars of weapons acquisition have piled on case-study upon case-study, giving our academic arsenals an "overkill" of histories of individual weapons systems, but relatively little accumulation of knowledge. More recently, broader and more comparative works, such as Evangelista's *Innovation and the Arms Race* (1988) and MacKenzie's *Inventing Accuracy* (1990), improved the field by explicitly testing the validity of different theoretical approaches across space and time. Graham Spinardi's intelligent and insightful book is a valuable contribution to this important trend. It is not only a wonderfully rich history of the evolution of a U.S. submarine launched ballistic missile (SLBM) systems, but at a more conceptual level, it demonstrates how a social construction of technology approach to weapons procurement can be used both to critique existing theories and to provide new insights.

Spinardi is at his best when debunking technological determinism. If the basic phenomenon consists of "technology out of control," then one should find a general progression of missile modernization over time, as more advanced technologies became widely available. One would also expect that great similarities would exist in the weapons systems developed by different military services, since each would be subject to the same forces of technological momentum. The evolution of U.S. missile systems contradicts these views. As Spinardi shows, the Navy's SLBMs contained relatively low yield warheads and remained relatively inaccurate through the 1960s and early 1970s, despite the existence of technological opportunities for greater accuracy and more counterforce "hard target kill" capability. It was only in the mid-1970s that Navy systems began to be developed with greater yields and accuracy. This contrasts sharply with the evolution of Air Force missiles, which starting from the late 1950s deployed large yield warheads with higher accuracies, which could be used for counterforce targeting.

It would be tempting to view this basic technological evolution, from the realist perspective, as the product of top-down guidance on national strategy. One could argue that the U.S. Navy saw that Secretary of Defense Robert McNamara placed his highest priority in maintaining Assured Destruction capabilities in the mid-1960s and stuck to the secretary's wishes, despite the Air Force's continuing intransigence. Moreover, one could argue that U.S. nuclear strategy changed in the 1970s, due to perceptions of a growing Soviet counterforce threat, and that the Navy changed its strategy under the new guidance derived from the 1974 Schlesinger Doctrine.

What is wrong with this view? Spinardi shows, quite compellingly in my view, that this explanation is simply not historically accurate. For example, the Navy developed and constrained the accuracy of the Polaris SLBM well before McNamara expressed his views on Assured Destruction. Spinardi, agreeing with the arguments of Sapolsky's *The Polaris System Development* (1972), also demonstrates how the missile's relatively inaccurate delivery capability was due more to a conscious policy of "differentiation" from the U.S. Air Force than from the inherent technical limitations stemming from underwater launches. The leaders of the Navy's Special Project Office (SPO) argued for a counter-city nuclear doctrine, and designed forces geared for that mission, not because they all believed that a doctrine made sense vis-a-vis the Soviet Union. (Indeed, the Navy leadership had earlier argued that such counter-city targeting was immoral and incredible.) They developed such systems because it made sense vis-a-vis the U.S. Air Force: emphasizing counter-city retaliation capabilities gave the Navy a unique mission and protected it from direct competition with its organizational rival.

Spinardi's close look at the evidence similarly shows that the Schlesinger Doctrine was not the key determinant of the shift in Navy policy regarding counterforce capabilities in the mid-to-late 1970s. After all, officials in the Office of the Secretary of Defense had for many years provided top-down guidance advocating greater counterforce capability in SLBMs, but to no avail as the SPO repeatedly would not meet these goals. What changed in the mid-1970s was that the differentiation strategy was no longer deemed to be in the Navy's best interests. During this period, the Air Force's MX missile program became increasingly threatened by cost overruns, Congressional concerns about "destabalizing" counterforce systems, and environmental opposition to its mobile basing modes. Once it became clear that the MX might not be fully funded, a broad coalition of Defense Department officials, Congressmen, and laboratory officials joined with the Navy leadership to support a fundamental change in Navy systems. Pentagon counterforce advocates wanted to ensure that hard-target kill improvements were not lost in case the MX was canceled; arms controllers in Congress saw an invulnerable counterforce SLBM as the lesser of two evils; and laboratory officials pushed for new, more advanced, warheads on any new missile system. In this context, Navy leaders saw an opportunity to promote their SLBM program as an alternative to the MX rather than protect it by keeping a different mission. As Spinardi nicely puts it, "the 'bureaucratic' logic for opposition to counterforce disappeared" (p. 151).

This argument obviously leaves Spinardi very close to the organizational-bureaucratic politics school. Does this mean the social construction approach is just bureaucratic politics with a new jargon? Certainly, much of the story Spinardi tells can be translated into a more traditional bureaucratic politics framework without much difficulty. For example, the "heterogeneous engineers" who manage the weapons programs in this book look a lot like good bureaucratic infighters to me. Still, Spinardi's social contructivist stance does provide valuable theoretical insights about the links between the political and technical sides of weapons acquisition.

Spinardi focuses our attention on the evolution of arguments used to support technology developments inside a bureaucratic organization, and shows how "mere justifications" can become transformed into genuine beliefs. While the Navy leadership changed its position to support counter-city targeting in the late 1950s and early 1960s as part of bureaucratic maneuver, by the 1970s a large number of naval officers involved in the programs had become intellectually wedded to this notion of "minimum deterrence." Indeed, it took the retirement of some senior officers at the Special Programs Office in order for the Navy leadership to push forward its preference for counterforce capabilities, and a number of lower-ranking personel immediately left the program in protest. Ideas take on a life of their own in large organizations. Perhaps individuals can keep separate books, remembering what are real motives and what are public justifications. But organizations have more limited flexibility, as members must be recruited, trained, and motivated with the same essential stories about the organization's purpose as those that are presented to the outside world.

Spinardi argues that his goal was to "reopen controversies or disagreements which have long since been resolved" (p. 16) so that particular weapons decisions no longer appear inevitable and once-possible alternatives are recognized. Politics and technology come together here. The physical world obviously presents severe barriers to technical possibilities; political "realities" similarly constrain the possible options open to bureaucratic actors. Yet, our perceptions of the locations and strengths of those barriers are shifting and strongly influenced by the interested parties. Spinardi repeatedly finds evidence of "heterogeneous engineers" arguing that certain technical and political options were not possible, when those options were simply not deemed desirable, and of actors insisting that options were possible, when they were at that point only desirable. In this important sense, Spinardi's book stands as a potent warning against the common tendency to assume that arguments about what is "technically possible" or "politically possible" always represent objective truths, rather than constructions that serve someone's interests.

Stanford University SCOTT D. SAGAN

Democracy and International Trade: Britain, France, and the United States, 1860–1990. By Daniel Verdier. Princeton: Princeton University Press, 1994. 387p. $45.00.

Scholars of international trade policy rarely attempt to explain all of history with one unifying theory. In *Democracy and International Trade,* Daniel Verdier not only offers us a new and novel theory to model most of U.S. trade policy (as did Judith Goldstein in *Ideas, Interests, and American Trade Policy,* 1993) but also applies it to explain British and French history as well. Verdier provides a historically rich and detailed account of trade policy for all three countries between 1860 and 1990, arguing that the electorate, which has been neglected in most research, plays an important role in determining the policy process (and thus, indirectly, policy outcomes). He argues that by electing legislators, voters both decide who makes policy and determine the boundaries and rules of the policymaking process. He maintains that the policy process in all three countries evolved over time in response to the will of voters and that this, more than any other theories of rent seeking, institutionalism, median voting models, or state autonomy, best explains trade policy outcomes.

In this theory of electoral delegation, the electorate defines the policy process by electing politicians who establish to whom the government will be accountable in trade policy matters. The choice of policy processes depends on the degree of salience and divisiveness among the electorate. By offering us this new approach, Verdier does not cast aside existing theories of rent seeking or the like. Rather, he shows how existing theories fit his larger framework of electoral control. For example, he claims that the United States over time moved from a period of interest group or pressure politics prominent in the postbellum years (1860–86) to a period of party politics, followed by the dominance of executive politics since World War II. In each of these periods, it is Verdier's contention that the electorate determined the type of policy process operating in trade politics. In other words, he maintains that the policy process itself is endogenous.

Though Verdier seemingly does a thorough job of tracing the developments in trade policy across time and countries, his theory does not always hold up well under a detailed scrutiny of the politics of policymaking. Take, for example, his claim that by the will of the electorate, executive politics has reigned in the U.S. from the end of World War II through the 1980s, "cushion[ing] trade policy against the vicissitudes of domestic politics" (p. 273). While it is true at some level that the executive has continued to play a dominant role in trade policy making, policymakers and the process did not successfully resist the protectionist onslaught as Verdier claims. On the contrary, special interests repeatedly succeeded in pressuring Congress to change the rules under which the executive could operate, thus successfully altering both the policy process and policy outcomes in their favor. The best current examples of this are the congressional changes made in GATT-sanctioned administered protection laws (antidumping, countervailing duty, etc.) that resulted in a huge increase in protection to a variety of special interest groups who successfully lobbied Congress for the changes. Indeed, in the 1980s, special industries and their associations even helped to write the new laws that Congress adopted to change the policy process. Alternatively, it could be argued that our legal obligations to the GATT and the importance of export markets to U.S. industrial sectors helped to mitigate the rise of pressure group influence over trade policy; but at no time in the 1980s is it clear that the electorate, rather than special interests, shaped U.S. policy. More plausible is the argument that in this period of integrated world markets, competing pressure groups led policymakers to choose a middle-of-the-road policy that offered protection for some but maintained the status quo for others. But the claim that special interests did not shape the policy process and help determine policy outcomes is simply not supported by the evidence.

A more important flaw in Verdier's argument is his apparent belief that the electorate is an independent player in shaping trade policy. To the contrary, the electorate itself has been largely influenced by the preferences of the owners of firms, who work hard to "educate" their employees and communities on the need for protection in order to succeed against their foreign competitors and preserve profits and jobs. This means that the will of the electorate is also endogenous.

Perhaps the greatest weakness in Verdier's study lies in his attempts to provide empirical support for his hypotheses. At times, the author's analyses (or his explanations) appear sloppy. For example, he uses ordinary least squares, rather than logit or probit analysis, on a dichotomous dependent variable (tbl. 11.1) and often does not explain clearly what his unit of analysis is. His analyses are also often inappropriate for the point that he is trying to make. For example, in chapters 8, 10, and 11 the author claims to analyze the determinants of tariffs by looking at *annual* changes in customs revenues; but (at least in the United States) tariff changes really only occurred at discrete points in time when Congress passed tariff legislation or the executive negotiated changes under the GATT. More appropriate models would have focused on these discrete tariff changes (as in tbl. 8.1) and the reasons for those changes in policy. As they stand, the author's analyses tell us very little about *policy change* and may instead be interpreted simply as models of consumption.

More importantly, in every chapter, the author offers empirical support for his hypotheses; yet at no point in the book does the author attempt to test competing hypotheses and theories simultaneously or to compare their impact on tariff rates, voting behavior, and the like. This lack of comparisons diminishes the validity of his theory and leads one to wonder whether his theory really does any better than the alternatives that he dismisses as endogenous to electoral control. His theory might perhaps be more believable if it stood the test of a direct comparison to competing theories. Nonetheless, Verdier does offer us a new and interesting way of looking at the formation of trade policy and a theory that transcends national borders in an attempt to explain policymaking in democracies more generally.

University of New Mexico WENDY L. HANSEN

Inside/Outside: International Relations as Political Theory. By R. B. J. Walker. New York: Cambridge University Press, 1992. 233p. $49.95.

Reality, it seems, is not what it used to be in international relations. The Cold War is over; and patterns of thought and behaviour identified as corresponding with an enduring, universal, "essence" of global existence are coming under increasing scrutiny as old ideological commitments and alliances are reformulated, territorial boundaries are hastily redrawn, and new symbols of identity are constructed and/or resurrected. Accordingly, critical dimensions are being added to an international relations literature that for almost 50 years, has resonated with one variation or another of a power-politics perspective on global political life, now represented by many as a restricted, inadequate, and highly dangerous anachronism in a complex, changing world. Acknowledging the great dangers of the current era, this new critical literature seeks also to take advantage of its opportunities for more incisive and less dangerous ways of thinking and acting in the space beyond the Cold War. It is in this context that R. B. J. Walker has made a significant and sustained contribution in recent years, and *Inside/Outside* represents the latest articulation of Walker's perspective on global affairs.

More precisely, *Inside/Outside* represents Walker's most concerted effort yet to illustrate to a contemporary readership the need to go beyond that dichotomy which at one level separates "domestic" from "international" practice and at another distinguishes something called political "theory" from its international counterpart. Indeed, he argues, one finds that it is precisely the connection between (domestic) political theory and international relations theory (between inside and outside) that has given international relations practice its dominant meanings and its way of framing the world in the contemporary era. As a consequence, the argument goes, this connection must be exposed, in detail, and its implications for theory and practice illustrated, in detail, if we are to begin to think and act in more open, more sophisticated, and less dogmatic ways in the 1990s.

In particular, Walker maintains, it is at the modernity intersection of Anglo–American political theory and international relations that the framing rituals associated with concepts of state-centrism, sovereignty, and political identity need to be reexamined if a broader, more incisive agenda of global life is to be realised in the post–Cold War era—hence the significance given in *Inside/Outside* to a rereading of major works on the question of modern social life (e.g., by Hobbes, Kant, Marx, and particularly Weber) and the manner in which these contributions to the traditions of political theory have been appropriated by international relations. Walker's conclusion, simply put, is that via a powerful strain of positivist logic integral to Anglo–American scholarship since the nineteenth century, the nuances and ambivalence of the modernity debate have been transformed into a crude, self-affirming realism that has, until recently, been effectively unchallenged within the international relations community. In taking up this challenge in *Inside/Outside,* Walker offers alternative ways of understanding contemporary global life and alternative representations of its dominant theory *as* practice (the inside as outside). He does so, primarily, in terms that indicate the influence upon his thinking of Foucault and Nietzsche, whose ambivalence toward modernity he utilizes to add interpretive dimensions to the taken-for-granted reference points of international relations scholarship. And he does so in a manner that is instructive to both advocates and critics of his (broadly) postmodernist perspective, illustrates that any deconstructive enterprise must be simultaneously reconstructive if it is to maintain its positive critical purpose, and speaks directly to issues often vaguely resonant in works of this ilk. Chapter 3, on the question of a modern political ethics, is particularly impressive in this latter context.

R. B. J. Walker has been one of the most innovative and incisive contributors to the expanded agenda on international relations over the past decade. *Inside/Outside* represents a working out of themes to be found in a range of places in this period. Indeed, seven of the eight chapters have appeared in one form or another elsewhere and this might understandably, inhibit some when it comes to buying the book or setting it for students. On the other hand, if one is seeking, in a single volume, a coherent broad-ranging and generally accessible articulation of some of the most important issues of theory/practice in the modern era, *Inside/Outside* fits the bill. As such, it deserves to be read widely and discussed seriously by an international relations community struggling to redefine itself and the world in a volatile age.

Australian National University JIM GEORGE

Political Science is BASIC

CANCER WARS

How Politics Shapes What We Know
and Don't Know About Cancer
ROBERT N. PROCTOR
"A brilliant history." —DOROTHY NELKIN, New York University
0-465-02756-3 $25.00

DEMOCRACY ON TRIAL

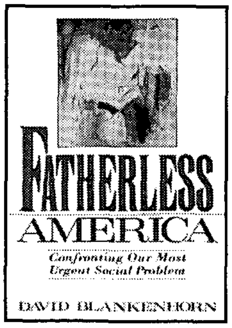
JEAN BETHKE ELSHTAIN
"A powerful moral statement about the meaning of democracy."
—ROBERT COLES
0-465-01616-2 $20.00

DENG XIAOPING

My Father
DENG MAOMAO
"Causing a sensation even before it is published." —*New York Times*
0-465-01625-1 $27.50

THE END OF VICTORY CULTURE

Cold War America and the
Disillusioning of a Generation
TOM ENGELHARDT
"One of those rare books that can change the way we see."
—TODD GITLIN, author of *The Sixties: Years of Hope, Days of Rage*
0-465-01984-6 $25.00

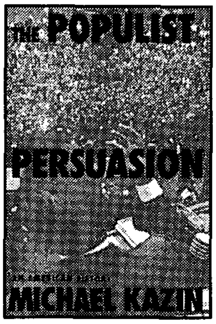


FATHERLESS AMERICA

Confronting Our Most
Urgent Social Problem
DAVID BLANKENHORN
"Well reasoned, empirically sound, and an important contribution to the public debate."
—WILLIAM J. BENNETT, author of *The Book of Virtues*
0-465-01483-6 $23.00

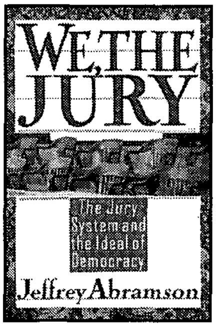
HISTORICAL CHANGE AND HUMAN RIGHTS

The Oxford Amnesty Lectures
EDITED BY OLWEN HUFTON
Contributors: Patrick Collinson, Emmanuel Le Roy Ladurie, Robert Darnton, Ian Kershaw, Elizabeth Fox-Genovese, Orlando Patterson, and Carlo Ginzburg
0-465-03076-9 $25.00


THE POPULIST
PERSUASION
MICHAEL KAZIN

THE POPULIST PERSUASION

An American History
MICHAEL KAZIN
"A tour de force. . . . A challenging analysis of one of the most important and enduring impulses of American political life."
—ALAN BRINKLEY, Columbia University
0-465-03793-3 $24.00


WE, THE
JURY
The Jury
System and
the Ideal of
Democracy
Jeffrey Abramson

WE, THE JURY

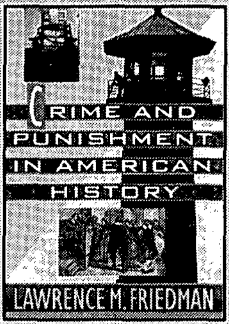
The Jury System and the
Ideal of Democracy
JEFFREY ABRAMSON
"This scholarly perspective is invaluable." —LINCOLN CAPLAN, *Washington Post Book World*
0-465-03698-8 $25.00

NEW IN PAPERBACK

Finalist, Pulitzer Prize in History

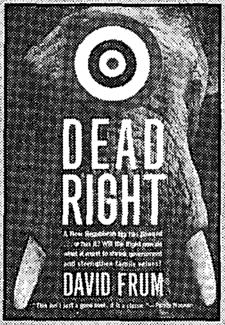
CRIME AND PUNISHMENT IN AMERICAN HISTORY

LAWRENCE M. FRIEDMAN
0-465-01487-9 $15.00



DEAD RIGHT

DAVID FRUM
0-465-09825-8 $12.00


DEAD
RIGHT
DAVID FRUM

THE END OF EQUALITY

MICKEY KAUS
0-465-09816-9 $12.00

LOSING GROUND

American Social Policy, 1950–1980
Tenth-Anniversary Edition
CHARLES MURRAY
0-465-04233-3 $16.00

Winner, Sidney Hillman Prize

NEVER STOP RUNNING

Allard Lowenstein and the Struggle to
Save American Liberalism
WILLIAM H. CHAFE
0-465-04985-0 $15.00

THE PROTEAN SELF

Human Resilience in an
Age of Fragmentation
ROBERT JAY LIFTON
0-465-06421-3 $13.00

BasicBooks
A Division of HarperCollins*Publishers*

At bookstores or call toll-free 800-331-3761

Also available from HarperCollins*CanadaLtd*

NEW

POLITICAL SCIENCE

TITLES

STEVEN WALDMAN

The Bill

How the Adventures of Clinton's National Service Bill Reveal What is Corrupt, Comic, Cynical, and Noble About Washington

An anatomy of a bill from stump-speech applause line through the labyrinth of Congress. 320 pp.

Viking 0-670-85300-3 $22.95

DARRELL DELAMAIDE

The New Superregions of Europe

"Delamaide's masterly, indispensable analysis of the emergent superregions is vital to understanding what's really happening in Europe."—Robert Heller. "An idea whose time is coming. As Delamaide shows vividly...regions must be the new focus for organizing the fragmented, complex continent."—Flora Lewis. "Penetrating. Presents a powerful and well-crafted analysis of major changes now reconfiguring Europe's economic landscape."—*USA Today*. 336 pp.

Plume 0-452-27255-6 $12.95

TODD G. BUCHHOLZ

From Here to Economy

A Short Cut to Economic Literacy

Buchholz tackles the subject of economic literacy by translating economist-speak into hip, everyday English for anyone who's affected by the economy but doesn't understand what the analysts are saying. "A painless, even entertaining, way to learn the basics. Buchholz knows his stuff."—Peter Passell, *The New York Times*. 288 pp.

Dutton 0-525-93902-4 $21.95

SUSAN ZAKIN

Coyotes and Town Dogs

Earth First! and the Environmental Movement

Coyotes and Town Dogs is a vivid account of the fascinating personalities—including Edward Abbey, David Brower, and John Muir—the flamboyant tactics, epic confrontations, and philosophical, legal, and ethical dilemmas of the entire American environmental movement, from grassroots activists to those in the corridors of power in Washington. 448 pp.

Penguin 0-14-014487-0 $12.95

ADAM HOCHSCHILD

The Unquiet Ghost

Russians Remember Stalin

"Absorbing....An important contribution to our post-glasnost awareness of the former Soviet Union's harrowing past—and of its unsettled present."—*Los Angeles Times Book Review*. "A perceptive, intelligent book demonstrating that the significance of the gulag transcends the confines of one country and one generation."—*The New York Times Book Review*. Photographs. 320 pp.

Penguin 0-14-015795-6 $11.95

JOHN MAYNARD KEYNES

The Economic Consequences of the Peace

Introduced by Robert Lekachman

"The most important economic document relating to World War I and its aftermath."—John Kenneth Galbraith. 336 pp.

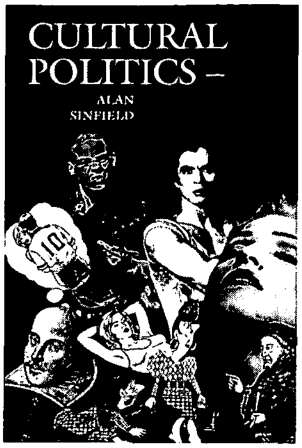
Penguin Twentieth-Century Classic
0-14-018805-3 $12.95

FOR A FREE HISTORY CATALOG, PLEASE CALL 212/366-2372

PENGUIN USA

ACADEMIC MARKETING DEPARTMENT • 375 HUDSON STREET • NEW YORK, NY 10014-3657

NEW FROM PENNSYLVANIA



Cultural Politics— Queer Reading

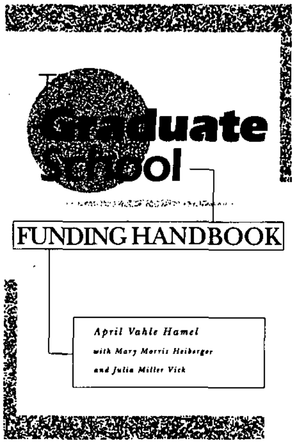
Alan Sinfield

"A book as colloquially direct and politically pungent as it is learned. It is brief but various and full of matter, and it contributes very usefully to our current discussions of literary and cultural theory and of the place of lesbian/gay studies in university study and political practice"—Henry Abelove.

Was Shakespeare gay? Is *The Merchant of Venice* anti-Semitic? How does mainstream reading differ from that of subcultural groups? In this lively and readable book, Alan Sinfield challenges the assumptions of English literature and investigates the principles and practices that may inform lesbian and gay reading. A volume in New Cultural Studies.

1994. 120 pp. Cloth, 3266-6, $28.95; paper, 1542-7, $11.95

OF SPECIAL INTEREST



The Graduate School Funding Handbook

April Vahle Hamel, with Mary Morris Heiberger and Julia Miller Vick

A comprehensive and easy-to-use manual designed to guide students through the intricate process of applying for graduate school funding both in the United States and abroad.

Jan. 1995. 176 pp. Cloth, 3232-1, $36.95; paper, 1447-1, $13.95

The Academic Job Search Handbook

Mary Morris Heiberger and Julia Miller Vick

"If you're looking for a job, chances are you have consulted one of the many how-to books available in your local library. But if you're a recently graduated Ph.D. looking for a job in an academic field, you haven't had a how-to book to consult. Until now"—*Philadelphia Inquirer*.

1992. 184 pp, 26 illus. Paper, 1375-0, $13.95

Human Rights of Women

National and International Perspectives

Edited by Rebecca J. Cook

The contributors to this volume analyze how international human rights law applies specifically to women in various cultures worldwide and develop strategies to promote equitable application of human rights law at the international, regional, and domestic levels. Their essays present a compelling mixture of reports and case studies from various regions in the world, combined with scholarly assessments of various aspects of international law as these rights specifically apply to women. A volume in Pennsylvania Studies in Human Rights.

1994. 656 pp, 2 illus. Cloth, 3261-5, $54.95; paper, 1538-9, $21.95

The Sports Franchise Game

Cities in Pursuit of Sports Franchises, Events, Stadiums, and Arenas

Kenneth L. Shropshire

In this innovative study, sports attorney Kenneth Shropshire describes the franchise warfare that pits city against city in the fierce bidding competition to capture a major league team. Rigorous research, fascinating interviews with the major players, stories behind the headlines, and an insider's perspective converge in this rare view of the business side of professional sports.

Jun. 1995. 120 pp. Cloth, 3121-X, $24.95

The United Nations Crime Prevention and Criminal Justice Program

Formulation of Standards and Efforts at Their Implementation

Roger S. Clark

In December 1991 the U.N. General Assembly adopted a resolution entitled "Creation of an Effective U.N. Crime Prevention and Criminal Justice Programme." Clark examines the history of the program and its prospects under the new arrangements devised by the General Assembly. Volume 20 in the Procedural Aspects of International Law Series.

1994. 352 pp. Cloth, 3269-0, $44.95

UNIVERSITY OF PENNSYLVANIA PRESS

To order books, call 1-800-445-9880

VISA & MasterCard accepted.

OXFORD UNIVERSITY PRESS

THE OXFORD ENCYCLOPEDIA OF THE
MODERN ISLAMIC WORLD

4-VOLUME SET

EDITED BY JOHN L. ESPOSITO

This unique new reference is the first comprehensive encyclopedia dedicated to the institutions, religion, politics, and culture in Muslim societies throughout the world. Placing particular emphasis on the nineteenth and twentieth centuries, the *Encyclopedia* contains over 750 articles in four volumes. The focus throughout is on the Islamic dimension of the Muslim experience in recent history, and background articles ground the information in its historical context and ensure continuity. Entries address political and social action as well as formal texts, and emphasis is placed on the practice and theory of Islam in Muslim societies around the globe. It is an invaluable resource offering extensive comparative and systematic analyses of Islamic beliefs, institutions, movements, practices, and peoples on an international scale.

March 1995 1936 pp.; 50 halftones $395.00 4-volume set

REAPPRAISING POLITICAL THEORY

REVISIONIST STUDIES IN THE HISTORY OF POLITICAL THOUGHT

TERENCE BALL

In this lively and entertaining book Terence Ball injects new life into the great works of political theory. By suggesting new ways of looking at the "classic" texts he shows how they can offer compelling answers to the problems we face today.

1995 328 pp. paper $19.95 cloth $59.00

WAR AND LAW SINCE 1945

GEOFFREY BEST

This important new book by a leading authority on ethics and war traces the recent history of these efforts, and explores key contemporary issues in the area. Best shows how the Second World War prompted reconstruction of international law, and charts the fortunes of its relations with war since then. He surveys the whole range of post-1945 conflicts to offer an original and thought-provoking approach to contemporary history, law, politics, and ethics.

1994 456 pp. $39.95

COMMERCIAL CULTURE

THE MEDIA SYSTEM AND THE PUBLIC INTEREST

LEO BOGART

"The most deeply informed and telling critique of mass culture in this timid time of political correctitude."—Robert K. Merton, *Columbia University*. The appetite for media, Bogart argues, differs from other demands the market is left to satisfy because it shapes the public's character and values. In this work he calls for a coherent national media policy, respectful of the American tradition of free expression, and subject to vigorous public debate.

1995 400 pp. $35.00

INCLUSIVE LEGAL POSITIVISM

W. J. WALUCHOW

This book develops a philosophical theory about the nature of law and its relationship with morality. Its central question is whether, in determining the extent of our legal rights and obligations, judges must appeal to moral principles.

1994 304 pp. $45.00

CAPITALISM WITH MORALITY

D. W. HASLETT

A timely study of the ethics of economic systems, this book uses both economic criteria and philosophically sophisticated standards to investigate ethical arguments. Areas covered include: ethical presuppositions; libertarian capitalism; and utopian communism.

1994 296 pp. $49.95

A POLITICAL THEORY OF RIGHTS

ATTRACTA INGRAM

Rights are basic building blocks of the contemporary state, yet their rigorous justification is difficult. This book provides a thorough analysis of this central topic, and challenges the orthodox view that rights are a type of property claim in one's body.

1995 248 pp. paper $29.95 cloth $59.00

THE WELFARE OF CHILDREN

DUNCAN LINDSEY

"The most interesting book on child welfare that I've read in the last ten years."—Neil Gilbert, *University of California at Berkeley*. Here, the author takes a critical look at the current child welfare system and its transformation into a system of child protection.

1994 416 pp. $29.95

THE IMAGINARY WAR

CIVIL DEFENSE AND AMERICAN COLD WAR CULTURE

GUY OAKES, *MONMOUTH COLLEGE*

"Well written and intelligent...."—Paul Boyer, *University of Wisconsin, Madison.* Here, the author contends that the real purpose of 1950's civil defense programs was not to protect Americans from the bomb, but to ingrain in them the moral resolve needed to face the hazards of the Cold War. It is essential reading for all those interested in American history, politics, and culture.

1995 208 pp. $29.95

THE GERMAN TRADITION OF ORGANIZED CAPITALISM

SELF-GOVERNMENT IN THE COAL INDUSTRY

MARTIN F. PARNELL

This work illustrates the historical evolution of the practice of industrial self-government and argues that historical continuities lie at the root of a full understanding of German capitalism. The study examines how intensive cooperation between state, management, private sector, and unions has shaped the industry both in growth and decline.

(Government-Industry Relations 7)

1994 280 pp. $55.00

ETHICS IN THE PUBLIC DOMAIN

ESSAYS IN THE MORALITY OF LAW AND POLITICS

JOSEPH RAZ

This new collection of essays forms a representative selection of his most significant contributions to a number of important debates, including the extent of political duty and obligation, and the issue of self-determination.

1994 376 pp. $60.00

THE CONSTITUTION AS POLITICAL STRUCTURE

MARTIN H. REDISH

This work emphasizes the central role served by the structural portions of the Constitution. Redish argues that these structural values were designed to provide the framework in which our rights-based system may flourish, and that judicial abandonment of these structural values threatens the very foundations of American political theory.

1995 240 pp. $39.95

CONSERVATIVE CENTURY

THE CONSERVATIVE PARTY SINCE 1900

EDITED BY ANTHONY SELDON AND STUART BALL

The dominant force in twentieth-century British politics, the Conservative Party has nevertheless been seriously neglected and misunderstood. *Conservative Century* systematically surveys the history of the Party from the "Khaki" election of 1900 to John Major's victory of 1992 and beyond.

1994 896 pp. $29.95

TRUE BLUES

THE POLITICS OF CONSERVATIVE PARTY MEMBERSHIP

PAUL WHITELEY, PATRICK SEYD, AND JEREMY RICHARDSON

"A rigorous and pathbreaking study...."—*The Economist.* This book challenges the stereotypical view of the Conservative activist as an eccentric extremist. Instead, the authors argue that the grass roots membership are the unsung heroes of political life, helping to keep the party system working at a time when it is under considerable strain.

1995 320 pp. paper $21.00 cloth $59.00

SIPRI YEARBOOK 1994

STOCKHOLM INTERNATIONAL PEACE RESEARCH INSTITUTE

This work continues SIPRI's review of the latest developments in nuclear weapons, world military expenditure, the international arms trade and arms production, chemical and biological weapons, the proliferation of ballistic missile technology, armed conflicts in 1993, and nuclear and conventional arms control.

1994 880 pp. $79.00

FOREORDAINED FAILURE

THE QUEST FOR A CONSTITUTIONAL PRINCIPLE OF RELIGIOUS FREEDOM

STEVEN D. SMITH

"Steven Smith is one of the most imaginative new voices in contemporary legal studies."—Michael J. Perry, *Northwestern University School of Law.* Contending that the perennial quest to distill religious freedom into a "principle" is futile, Smith advocates a fundamental reassessment of the premises upon which courts have proceeded in this area.

1995 192 pp. $35.00

THE FRENCH COMMUNIST PARTY IN THE FIFTH REPUBLIC

D. S. BELL AND BYRON CRIDDLE

Here, the authors trace the history of the Communist Party in France from its origins, focusing on the period since 1958 and exploring the Party's organizational structure and international loyalties. They examine structure and ideology, relations with the Socialist party, electoral performance, and the 1980's decline in the Party's fortunes.

1994 312 pp. $55.00

FREE EXPRESSION

ESSAYS IN LAW AND PHILOSOPHY

EDITED BY W. J. WALUCHOW

This collection of essays originated at a conference on freedom of expression held at McMaster University in 1990. Its contributors are philosophers and lawyers who each bring a unique perspective to bear on issues surrounding the justification of free expression and the bases, both legal and moral, for restricting or broadening its scope.

1994 280 pp. $55.00

Prices are subject to change and apply only in the U.S. To order by phone using major credit cards, please call 1-800-451-7556. In Canada, please call 1-800-387-8020.

200 MADISON AVENUE • NEW YORK, NY 10016

Scholarly & Reference Division

Ideas That Shape Politics

Edited by **Michael Foley**

This is a handbook of political thinking for the late twentieth century. In a series of over twenty original essays, distinguished writers analyze political ideas according to their capacity to motivate and justify political action.

1994 • 240 pp.• $69.95 cl.• $19.95 pb.• Manchester University Press

Radical Theories

Paths Between Marxism and Social Democracy

Darrow Schecter

This book aims to reclaim and rediscover the range of radical, democratic, socialist alternatives to capitalism. Darrow Schecter argues that while the collapse of the Soviet Union has seen the failure of one type of socialism, it has presented the left with the chance to re-evaluate the contribution of thinkers and movements obscured by the hegemony of Marxism-Leninism.

1994 • 224 pp.• $69.95 cl.• $29.95 pb.• Manchester University Press

Islam and Politics in Central Asia

Mehrdad Haghayeghi

Many observers have lazily concluded that the new Central Asian states will become filled with hellfire mullahs overnight simply because they border Iran. But the diversity and tolerance displayed in the region have belied such predictions. This book shows that, while information about the region was limited during the Soviet period, it is now vital to obtain a more sophisticated understanding of these inter-related countries.

April 1995 • 320 pp.• $45.00 cl.

Islamism and Secularism in North Africa

Edited by John Ruedy

Leading scholars from the United States, Europe, and Africa explore the Islamic challenge in North Africa. Drawing on various disciplines, they examine the relations among religion, the state, and political opposition movements.

1994 • 288 pp.• $49.95 cl.• Published in cooperation with the
Center for Contemporary Arab Studies, Georgetown University

Election '94 South Africa

The Campaigns, Results and Future Prospects

Edited by **Andrew Reynolds**

With chapters from leading South African and international experts, this book captures the character and key events of the April 1994 election campaign in South Africa. It provides an authoritative analysis of the election results, and looks at the prospects for the future of greater understanding of this landmark event.

1994 • 237 pp.• $39.95 cl.

Improbable Dangers

U.S. Conceptions of Threat in the Cold War and After

Robert H. Johnson

Robert Johnson focuses on the U.S. conceptions of threat in the European, nuclear, and Third World arenas of conflict. This is a different kind of Cold War revisionism, concentrating on mistaken ideas about threats while accepting the reality of threat and the need for a policy of containment.

1994 • 320 pp.• $39.95 cl.

A History of Slovakia

The Struggle for Survival

Stanislav J. Kirschbaum

Stanislav Kirschbaum describes the history of the Slovak nation from its arrival in the Danubian Plain and the valleys of the Tatra Mountains to the declaration of independence in 1993.

January 1995 • 368 pp.• $49.95 cl.

Violence and Diplomacy in Lebanon

The Troubled Years, 1982-1988

Elie Salem

Elie Salem, former foreign minister and advisor to the Lebanese president, has written an insider's memoir of Lebanon's troubled years when killings and kidnappings isolated the population from the rest of the world.

January 1995 • 256 pp.• $35.00 cl.• I.B.Tauris & Co.

Khrushchev

A Political Life

William J.Tompson

This is a biography of Nikita Sergeeviech Khrushchev, tracing his life and career. The book concentrates on his years in power (1953-1964) and his role in shaping the development of the Soviet Union after Stalin.

January 1995 • 300 pp.• $24.95 cl.

Whither Israel?

The Domestic Challenge

Edited by **Keith Kyle** and **Joel Peters**

As it enters the 1990s, Israel faces crucial political, economic and social challenges. Keith Kyle and Joel Peters consider the impact of these problems and their implication for the future direction of Israeli politics and society.

1994 • 304 pp.• $19.95 pb.

257 Park Avenue South • New York, NY 10010 • 1-800-221-7945

CALIFORNIA

POLITICAL SCIENCE

Richard Madsen

China and the American Dream

A Moral Inquiry

Madsen's frank and innovative examination of the moral history of U.S.-China relations targets the forces that have shaped this surprisingly strong tie between two strikingly different nations.

A Philip E. Lilienthal Book, $35.00 cloth

Jonathan Marshall

To Have and Have Not

Southeast Asian Raw Materials and the Origins of the Pacific War

"An outstanding contribution to understanding the road to World War II in the Far East . . . Enough interesting detail to move even the strongest skeptic."
—Laurence H. Shoup, author of *The Carter Presidency and Beyond*

$28.00 cloth

Ellen Herman

The Romance of American Psychology

Political Culture in the Age of Experts

"A wonderfully written book . . . [about] a little-recognized but enormously significant process that has shaped contemporary American political culture." —Cynthia Enloe, author of *The Morning After*

$35.00 cloth, illustrated

At bookstores or order 1-800-822-6657

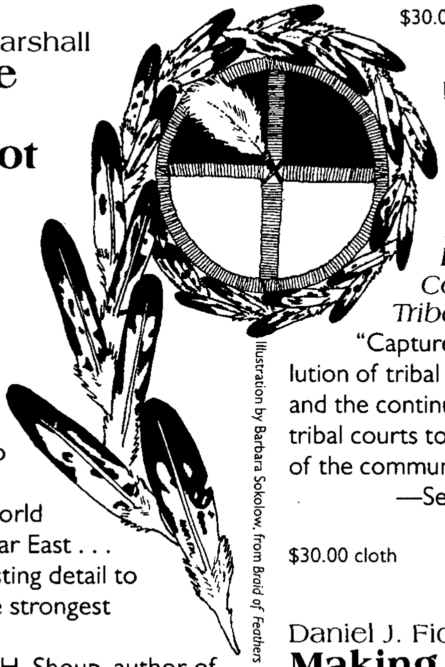


Illustration by Barbara Sokolow, from *Braid of Feathers*

Sam Davis

The Architecture of Affordable Housing

Here is an accessible, non-technical analysis for everyone interested in the creation of affordable housing. Through discussions of cost, politics, and design concepts, Davis offers solutions to the dilemmas posed by the development process.

$30.00 cloth, illustrated

Frank Pommersheim

Braid of Feathers

American Indian Law and Contemporary Tribal Life

"Captures the unique evolution of tribal court jurisprudence and the continuing challenge facing tribal courts to meet the demands of the communities they serve."
—Senator Ben Nighthorse Campbell, Colorado

$30.00 cloth

Daniel J. Fiorino

Making Environmental Policy

Fiorino provides the fullest, most readable introduction to federal environmental policymaking yet published.

$40.00 cloth, $15.00 paper, illustrated

Daniel M. Fox

Power and Illness

The Failure and Future of American Health Policy

New in paper with a New Preface—"An astute, insider's view of health politics, past and present."
—*San Francisco Chronicle Review*

$12.00 paper, illustrated

Lena Constante

The Silent Escape

Three Thousand Days in Romanian Prisons

Translated by Franklin Philip
Introduction by Gail Kligman

"A heartbreaking, human document . . . that ranks comparably to Ginzburg's *In the Whirlwind* and Mandelstam's *Hope Against Hope*."
—Vladimir Tismaneanu, University of Maryland, College Park

Society and Culture in East-Central Europe, $22.00 cloth

Stephen Kotkin

Magnetic Mountain

Stalinism as a Civilization

"A kind of archaeological analysis of Soviet life during the momentous years of Stalinist industrialization."
—Lewis Siegelbaum, Michigan State University

$55.00 cloth, illustrated

David G. Gutiérrez

Walls and Mirrors

Mexican Americans, Mexican Immigrants, and the Politics of Ethnicity

Gutiérrez examines the ways that continuous immigration from Mexico transformed— and continues to shape—the political, social, and cultural life of the American Southwest.

$40.00 cloth, $15.00 paper

William Finnegan

Dateline Soweto

Travels with Black South African Reporters

New in paper—"Neither sensationalizes nor patronizes. . . . A concrete, precise textbook of sorts, with drama, provocative ideas, new perspectives and rarely heard voices added."—*New York Times Book Review*

$12.00 paper

UNIVERSITY OF CALIFORNIA PRESS

New in VIRGINIA'S Constitutionalism and Democracy Series

New Communitarian Thinking

Persons, Virtues, Institutions, and Communities

Edited by AMITAI ETZIONI

Communitarian thought is at the heart of a fierce debate in political theory about the justice, efficacy, and future of liberalism and liberal societies. Amitai Etzioni has collected a sterling list of contributors who bring communitarian thinking to bear on such timely and contentious issues as abortion, homosexuality, free speech, and personal autonomy.

May, $55.00 cloth, $18.50 paper

The Martinsville Seven

Race, Rape, and Capital Punishment

ERIC W. RISE

"This should be the definitive study of the Martinsville Seven Case. Rise skillfully places the case in its broader Virginia and national context, demonstrating how the trial raised emotional issues of crime control, race relations, gender relations, and federalism. He is particularly effective in discussing the struggle between the NAACP and the CRC over control of the appeals."
—James W. Ely, Jr., Vanderbilt University

June, $27.95

To Build a Wall

American Jews and the Separation of Church and State

GREGG IVERS

"To my knowledge, *To Build a Wall* is the only careful study and extensive treatment of the role of the three Jewish organizations in church/state litigation. The scholarship is first rate in this book. . . . [Ivers] has mastered his subject and demonstrates an almost flawless sense of the relationships that prevail among the various cases and causes he addresses."**—Robert Alley, University of Richmond**

June, $37.50 cloth

Also of Interest

Dividing the Commons

Politics, Policy, and Culture in Botswana

PAULINE E. PETERS

"*Dividing the Commons* is impressive in its command of problems of development policy, history, ethnography, and social theory. It offers innovative and penetrating analysis of a number of key scholarly and policy controversies and takes a critical historical look at the lauded 'democratic' character of contemporary Botswana."
—Angelique Haugerud, Yale University

$49.50 cloth, $18.95 paper

French and West Indian

Martinique, Guadeloupe, and French Guiana Today

Edited by RICHARD E. BURTON and FRED RENO
Foreword by A. James Arnold
New World Studies Series

This book assesses the effects of almost half a century of political assimilation into France and asks to what extent the high standard of living enjoyed by French West Indians today has been offset by losses on the political, cultural, and psychological levels.

March, $42.50 cloth, $15.00 paper

UNIVERSITY PRESS OF VIRGINIA

Box 3608 University Station • Charlottesville, VA 22903-0608
804-924-3469/ FAX 804-982-2655/ e-mail: upressva@virginia.edu

PRINCETON UNIVERSITY PRESS

DEMOCRATIZATION IN SOUTH AFRICA

The Elusive Social Contract

Timothy D. Sisk

Timothy Sisk presents a new way of conceiving the transition to democracy in South Africa. Unlike authors who have sought to prescribe an ideal set of post-apartheid political institutions, Sisk asks what kinds of institutions show signs of actually emerging, given recent history and present realities. He treats the problem of constructing a democratic post-apartheid society in South Africa as part of a larger condition common to societies deeply divided by ethnic, religious, racial, or national discord.

"Sisk combines theoretical sophistication with compelling supportive empirical data. This is a timely and significant contribution to the study of ethnic and racial relations in general and South African politics in particular. Must reading for anyone concerned with constitutional engineering or interracial and interethnic relations."
—Donald Rothchild, University of California, Davis

Cloth: $39.50 ISBN 0-691-03622-5

DEMOCRACY FROM SCRATCH

Opposition and Regime in the New Russian Revolution

M. Steven Fish

This book presents a fresh view of Russian political change in the Gorbachev and early post-Soviet periods not by examining *perestroika* and *glasnost* in and of themselves, but by investigating the autonomous political organizations that responded to liberalization. Extensive study of these political groups, in Moscow and several provincial cities, has led M. Steven Fish to conclude that they were shaped to a far greater degree by the nature of the Soviet state than by socioeconomic modernization, political culture, native psychology, or Russian historical tradition.

This book proposes a novel and theoretically sophisticated way to study Russian politics and offers a rigorous approach to understanding social movements, political party formation, regime change, and democratization in general.

"*Democracy from Scratch* is an excellent comparative political study and the best book on post-communist Russian politics that I have read. It will doubtless become a standard in the field and will probably exert a considerable influence in shaping the field itself."—Michael Urban,
University of California, Santa Cruz

Cloth: $39.50 ISBN 0-691-03703-5

PUBLIC ENTREPRENEURS

Agents for Change in American Government

Mark Schneider and Paul Teske with Michael Mintrom

Seizing opportunities, inventing new products, transforming markets—entrepreneurs are an important and well-documented part of the private sector landscape. Do they have counterparts in the public sphere? The authors argue that they do, and test their argument by focusing on agents of dynamic political change in suburbs across the United States, where much of the entrepreneurial activity in American politics occurs.

How do public entrepreneurs emerge? How much does the future of urban development depend on them? This book answers these questions, using data from over 1,000 local governments. The authors augment their quantitative analysis with ten case studies.

"This is a significant piece of scholarship addressing how governments undergo change, specifically change that is initiated by individuals and not dependent on exogeneous forces."—Robert Stein, Rice University

Cloth: $39.50 ISBN 0-691-03725-6 *Due January 1995*

New in paperback

APPLE OF GOLD

Constitutionalism in Israel and the United States

Gary Jeffrey Jacobsohn

By comparing the constitutional systems of Israel and the United States, Gary Jacobsohn provides a new view of the essentials of constitutionalism itself.

"Gary J. Jacobsohn's book is among the most astute and far-seeing of the current batch of books on comparative constitutional issues. In fact, it is hard to overstate the importance of his book because its significance transcends its immediate subject. He compares Israel and the United States, but his comparison speaks powerfully to the identity crisis of liberalism in a multicultural era."—Graham Walker, *American Political Science Review*

"Jacobsohn's comparative study of constitutionalism in the United States and Israel is a model of comparative constitutional analysis, and the best book of its kind in a very, very long time."—John E. Finn, *The Review of Politics*

Paper: $14.95 ISBN 0-691-02953-9

PRINCETON UNIVERSITY PRESS

AVAILABLE AT FINE BOOKSTORES OR DIRECTLY FROM THE PUBLISHER: 800-777-4726

INTRODUCING

The United Nations
Blue Books Series

—— comprehensive and authoritative insights into the work of the Organization.
The "**United Nations Blue Books Series**" is a unique collection that covers major developments in international affairs in which the United Nations played a central and determining role.

The following books in the Series have just been released:

The United Nations and Apartheid
1948-1994

This landmark publication chronicles the central role played by the United Nations in supporting the struggle against apartheid in South Africa.

In an extensive introduction, UN Secretary-General Boutros Boutros-Ghali provides an overview of the Organization's contribution to South Africa's historic transformation. In addition, the publication includes the text of more than 200 key documents. These are supported by indexes, a detailed chronology and a bibliography of United Nations documentation, making this an essential reference work for anyone interested in the long fight against apartheid or in the work of the United Nations in helping to resolve one of the major issues of the century.

E.95.I.7 92-1-100546-9
572pp $29.95

The United Nations and Cambodia
1991-1995

This essential reference work chronicles the central role played by the United Nations Transitional Authority in Cambodia (UNTAC) in that country's struggle to democracy.

In an extensive introduction, UN Secretary-General Boutros Boutros-Ghali offers a comprehensive account of the Organization's contribution in supporting the peace process and organizing the landmark 1993 elections that led to the formation of a new, democratically elected Cambodian Government. This is complemented with texts of more than 100 key United Nations documents which include:

- ■ Resolutions of the Security Council
- ■ Reports of United Nations Secretaries-General
- ■ The 1991 historic agreements signed in Paris

E.95.I.9 92-1-100548-5
360pp. $29.95

This Series is designed to facilitate primary research and reference tools to academics, policy makers, journalists and others interested in gaining a deeper understanding of the work of the United Nations.

Other titles in the Series include:

- ▲ The United Nations and The Nuclear Non-Proliferation Treaty
- ▲ The United Nations and El Salvador, 1990-1995
- ▲ The United Nations and Mozambique, 1992-1995
- ▲ The United Nations and Human Rights, 1948-1995

Send orders to:

United Nations Publications, Room DC2-0853,
Dept. 210A, New York, N.Y. 10017
Tel. (800) 253-9646, (212) 963-8302,
Fax. (212) 963-3489.
Visa, MC and AMEX accepted. Please add 5% of gross ($3.50 minimum) for shipping and handling.

OXFORD UNIVERSITY PRESS

Now in a new edition!

Freedom and the Court

Civil Rights and Liberties in the United States

Sixth Edition

HENRY J. ABRAHAM, *University of Virginia*, and BARBARA A. PERRY, *Sweet Briar College*

"This wonderful book shows why Henry Abraham was recently awarded the 1st Lifetime Achievement Award by the Public Law section of the American Political Science Association. It also shows that Prof. Abraham is not content to simply rest on his laurels. Prof. Perry should be commended as well for bringing some fresh insights to a classic text."—Scott Gerber, *College of William and Mary*

1994 504 pp. paper $25.00

World Resources 1994-1995

WORLD RESOURCES INSTITUTE In collaboration with The United Nations Environment Programme and The United Nations Development Programme

"An excellent reference on environment, population, and development. Short essays give a well balanced review of recent trends, growing global concerns, and possible future impacts."—Mohan N. Shrestha, *Bowling Green State University*

1994 400 pp.; illus. paper $21.95

Human Development Report 1994

UNITED NATIONS DEVELOPMENT PROGRAMME

"Just what students in 1994-95 must know to survive in a globalized economy."—E. Kelly, *California State University at Sacramento*

1994 240 pp.; 29 illus. paper $17.95 cloth $29.95

The Formation of American Local Governments

Private Values in Public Institutions

NANCY BURNS, *University of Michigan*

"A very important study. Provides original insight about a major aspect of American urban politics that has been largely neglected by modern scholarship. The findings are interesting, the methodologies are appropriate and the theoretical implications are most stimulating. Will draw widespread attention from professionals, scholars, and students of local politics."—Paul Kantor, *Fordham University*

1994 208 pp.; 13 illus. paper $15.95 cloth $39.95

Building a New Global Order

Emerging Trends in International Security

Edited by DAVID DEWITT, *York University*, DAVID HAGLUND, *Queen's University, Kingston, Ontario*, and JOHN KIRTON, *University of Toronto*

"A multitude of perspectives...incorporates a truly international view. Scholars of international security will certainly find it to be an exciting and profound treatment of both new and perennial issues."—*Canadian Institution of International Affairs*

1994 440 pp.; 9 figs., 21 tables paper $19.95

Strategic Public Diplomacy and American Foreign Policy

The Evolution of Influence

JAROL B. MANHEIM, *George Washington University*

"It's great! I will add it to the required reading list for all my political communications graduate students."—Lance Bennett, *University of Washington*

1994 224 pp.; 19 illus. paper $14.95 cloth $39.95

International Organization and Industrial Change

Global Governance Since 1850

CRAIG N. MURPHY, *Wellesley College*

"A bold interpretation of the history of international organizations as expressions of social forces—'historical blocs' in the theory of Antonio Gramsci. Murphy argues that international organizations have fostered industrial change under capitalism by facilitating transportation and communication, and by carrying the ideals of liberal internationalism. He suggests that international institutions reflecting a new global ecological Keynesianism could promote the further diffusion of technology and dampen conflict between the rich and the poor. A challenging and original work."
—Robert O. Keohane, *Harvard University*

(Europe and the International Order)

1994 352 pp.; 7 figures paper $17.95 cloth $39.95

Deadlock or Decision

The U.S. Senate and the Rise of National Politics

A Twentieth Century Fund Book

FRED R. HARRIS, *University of New Mexico*

"Offers the reader a rare glimpse into the inner workings of the senate from a political scientist who actually was a member of the senate for several years. His book, therefore, has an aura of authenticity about it that should prove of value to students."—Robert Langran, *Villanova University*

1993 360 pp. paper $14.95 cloth $27.50

The President and the Parties

The Transformation of the American Party System Since the New Deal

SIDNEY M. MILKIS, *Brandeis University*

"For anyone interested in how Franklin Roosevelt and the New Deal shaped the American party system, this will be required reading. Milkis argues, convincingly, that Roosevelt made an eyes-open choice to build an administrative presidency rather than rule through party government."—David Mayhew, *Yale University*

1993 424 pp. paper $19.95 cloth $49.95

How Women Legislate

SUE THOMAS, *Georgetown University*

"A first-rate book by an up-and-coming young political scientist, likely to be the standard for some time....Interesting and important."—Barbara Sinclair, *University of California, Riverside*

1994 224 pp. paper $14.95 cloth $35.00

Prices are subject to change. To request an examination copy, write on school letterhead giving full course information, including course name, level, expected enrollment, and your decision deadline, to:

Oxford University Press
ATTN: College Sales Coordinator

200 MADISON AVENUE • NEW YORK, NY 10016

THE BEST IN POLITICS

Contexts of International Politics

Gary Goertz

This book examines the ways and means by which nation-states interact with their environment. The author proposes a theory of context that examines the way in which system structure, normative environment, and history can act as contexts for decision makers, either as causes, barriers, or by changing meanings. The theoretical and methodological material is illustrated by a number of case studies that apply context theory to specific problems, such as decolonization, USSR-East European relations, and enduring military rivalries.

Cambridge Studies in International Relations 36

44070-X Hardback $59.95 / 46972-4 Paperback $21.95

The Enlightenment's 'Fable'

Bernard Mandeville and the Discovery of Society

E. J. Hundert

The apprehension of society as an aggregation of self-interested individuals was first systematically articulated during the Enlightenment. This book examines the challenge offered to traditions of morality and social understanding by Bernard Mandeville, whose infamous maxim "private vices, public benefits" profoundly disturbed his contemporaries, while his *The Fable of the Bees* influenced David Hume, Jean-Jacques Rousseau, Adam Smith and Immanuel Kant.

Ideas in Context 31

46082-4 Hardback $54.95

The Collapse of a Single Party System

The Disintegration of the Communist Party of the Soviet Union

Graeme Gill

Following the debates in the Communist party over the implications of Gorbachev's reforms, this in-depth analysis of its disintegration (1985–1991) reveals the institutional dynamics of a party under pressures that ultimately led to the collapse of the most powerful single-party state in history.

Cambridge Russian, Soviet and Post-Soviet Studies 94

46537-0 Hardback $59.95 / 46943-0 Paperback $18.95

Russia in Search of its Future

Amin Saikal and William Maley, Editors

Marked by escalating conflicts, insurrection and street violence, the status of Russia's renewal since the collapse of communism remains uncertain. These contributions analyze a Russia that is searching for its future amid a maelstrom of complex forces—political, economic, sociocultural, and international.

Contributors: *Amin Saikal, William Maley, A. V. Obolonsky, Archie Brown, Robert F. Miller, Stephen Fortescue, Leslie Holmes, Sergei Serebriany, Peter Sawzak, John Miller, T. H. Rigby*

48260-7 Hardback $49.95 / 48387-5 Paperback $14.95

Islamic Society and State Power in Senegal

Disciples and Citizens in Fatick

Leonardo A. Villalon

The Sufi Muslim orders are the most significant institutions in Senegalese society. While Islamic political groups are often accused of destabilizing African states, Villalon argues that these brotherhoods have played a crucial part in making Senegal one of the most stable and democratic of African countries. Focusing on a regional administrative center, he combines a detailed account of grassroots politics with an analysis of national and international political forces.

African Studies Series 80

46007-7 Hardback $59.95

Judging the State

Courts and Constitutional Politics in Pakistan

Paula R. Newberg

In an original study of the relationship between state and civil society in Pakistan, Newberg demonstrates how, over the course of almost five decades, the courts have influenced the development of its constitutions and the state structure. By examining judicial decisions, she considers how tensions within the judiciary, and between courts and other state institutions, have affected the ways political society views itself, and explores the effects of these debates on the organization of political power.

Cambridge South Asian Studies 59

45289-9 Hardback about $54.95

Bengal Divided

Hindu Communalism and Partition, 1932–1947

Joya Chatterji

In an original and compelling account of the political and social processes that led to the demand for Partition in Bengal, the author traces the rise of Hindu communalism and shows how a large and powerful section of Hindu society in Bengal insisted that their province be divided so as to create a separate Hindu homeland. The picture that emerges is one of a stratified and fragmented society moving away from the mainstream of Indian nationalism, and increasingly preoccupied with narrower and more parochial concerns.

Cambridge South Asian Studies 57

41128-9 Hardback $59.95

Available in bookstores or from

CAMBRIDGE
UNIVERSITY PRESS

40 West 20th Street, New York, NY 10011-4211
Call toll-free 800-872-7423
MasterCard/VISA accepted. Prices subject to change.

THE BEST IN POLITICS

Faded Dreams

The Politics and Economics of Race in America

Martin Carnoy

"The best book written on the subject in the past dozen years. It is based upon a wealth of original research on racial and ethnic income differences that Carnoy has been carrying out in recent years. He is a recognized international leader in these areas, and the chapters on economic trends are particularly well done. In sum, this is a very important book that should become quite influential."
—Michael Reich, University of California, Berkeley

47062-5 Hardback $29.95

Lyndon Johnson Confronts the World

American Foreign Policy, 1963–1968

Warren I. Cohen and
Nancy Bernkopf Tucker, Editors

This comprehensive, perceptive review of the foreign policy of the Lyndon Johnson era demonstrates U.S. concern not only with the Soviet Union, Europe, and nuclear weapons issues, but the overwhelming preoccupation with Vietnam that shaped policy throughout the world.

Contributors: *Warren I. Cohen, Waldo Heinrichs, Walter LaFeber, Richard H. Immerman, Nancy Bernkopf Tucker, Robert J. McMahon, Frank Costigliola, Joseph S. Tulchin, Terrence Lyons*

41428-8 Hardback $54.95 / 42479-8 Paperback $16.95

Dismantling the Welfare State?

Reagan, Thatcher and the Politics of Retrenchment

Paul Pierson

This book offers a careful examination of the politics of social policy in an era of austerity and conservative governance. Focusing on the administrations of Ronald Reagan and Margaret Thatcher, Pierson provides a compelling explanation for the welfare state's durability and for the few occasions where each government was able to achieve significant cutbacks. The book will appeal to those interested in the politics of neo-conservatism as well as those concerned about the development of the modern welfare state.

Cambridge Studies in Comparative Politics

40382-0 Hardback $49.95

The Puzzle of Strikes

Class and State Strategies in Postwar Italy

Roberto Franzosi

Focusing on the strategic interaction among workers, employers, and the state, this book examines the temporal movement of postwar Italian strikes. Incorporating several theoretical approaches and based on many forms of empirical evidence (statistical, historical, ethnographic, and survey), *The Puzzle of Strikes* is unique in its broad concern with a variety of factors, theories, and forms of empirical evidence.

Cambridge Studies in Comparative Politics

45287-2 Hardback $54.95

Government Survival in Parliamentary Democracies

Paul V. Warwick

A highly puzzling phenomena in politics is why some parliamentary democracies manage to produce highly durable governments whereas others experience governmental instability. This book investigates this issue, using an innovative statistical methodology and a new data set covering sixteen West European countries over the entire postwar period. The results fundamentally challenge current theorizing on government survival and point to an alternative perspective on the relationship among governments, parties, and voters.

47028-5 Hardback $49.95

Cabinet Ministers and Parliamentary Government

Michael Laver and
Kenneth A. Shepsle, Editors

Little is known about the relationship between legislature and executive in parliamentary regimes. In this much-needed volume, leading specialists in the major parliamentary democracies describe and analyze legislative-executive interactions in terms of a common theoretical framework.

Contributors: *Michael Laver, Kenneth A. Shepsle, Wolfgang Muller, Kaare Strom, Rudy B. Andeweg. Wilma Bakema, Brian Farrell, Jaakko Nousianinen, Arco Timmermans, Francois Petry, Jean-Louis Thiebault, Ferdinand Muller-Rommel, Torbjorn Larsson, Annarita Criscitiello, Anthony King, Matthew S. R. Palmer, Graham White, Kleomenis S. Koutsoukis*

Political Economy of Institutions and Decisions

43246-4 Hardback $59.95 / 43837-3 Paperback $21.95

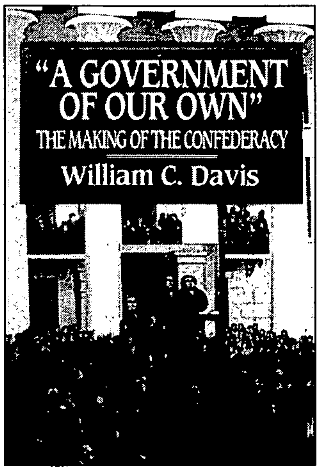
Citizens, Politics, and Social Communication

Information and Influence in an Election Campaign

Robert Huckfeldt and *John Sprague*

This book investigates the political implications of interdependent citizens within the context of the 1984 presidential election campaign as it was experienced in the metropolitan area of South Bend, Indiana. Several different themes are explored: the dynamic implications of social communication among citizens, the importance of communication networks for citizen decision-making, the exercise of citizen purpose in locating sources of information, the constraints on individual choice, and institutional and organizational effects.

45298-8 Hardback $49.95



"A GOVERNMENT OF OUR OWN"

The Making of the Confederacy

William C. Davis

“Davis brings a mastery of sources and splendid narrative gifts to the subject of political events in Montgomery, Alabama, during the momentous first five months of 1861. A compelling intersection of personalities and issues comes vividly alive in this exploration of a fledgling nation's quest for independence. The opening act of the Confederate experiment has never been examined in such depth or related with such drama.”
—**Gary W. Gallagher,**
Penn State University

1994 0-02-907735-4 $27.95

THE SOVIET TRAGEDY

A History of Socialism in Russia, 1917-1991

Martin Malia

“In Martin Malia, the Soviet Union had one of its most acute observers. With this book, it may well have found the cornerstone of its history.”
—**Francois Furet,** author of
Interpreting the French Revolution

1994 0-02-919795-3 $24.95

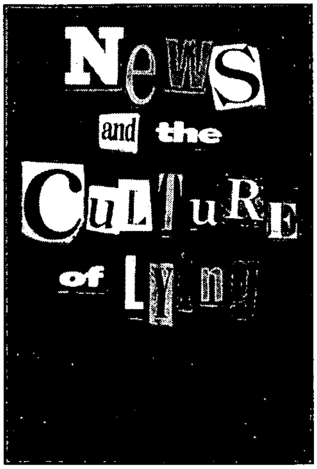
EXILE'S RETURN

The Making of a Palestinian-American

Fawaz Turki

“One of the best books I've ever read about the struggles of an individual choosing between traditional and modern society. A compelling and very readable work which gives superb insights into the Palestinian situation in all its complexity.”
—**Barry Rubin,** author of
Paved With Good Intentions: The American Experience in Iran

1994 0-02-932725-3 $22.95



NEWS AND THE CULTURE OF LYING

How Journalism Really Works

Paul H. Weaver

“Drawing on the author's own experience as a journalist and deftly dissecting the character of news and newswork, this engaging essay—part confession, part history, part polemic—deserves wide attention and announces Paul Weaver as one of our most penetrating critics of journalism.”
—**Michael Schudson,** University of California, Los Angeles

1994 0-02-934021-7 $22.95

PITIED BUT NOT ENTITLED

Single Mothers and the History of Welfare

Linda Gordon

“Linda Gordon's thorough look at the history of single mothers and welfare policy goes a long way towards shedding light on these misunderstandings and clearing the path to real reform.”
—**Representative Lynn Woolsey,**
(D-California)

1994 0-02-912485-9 $22.95

ORWELL'S REVENGE

The 1984 Palimpsest

Peter Huber

“Prophesying the future with a style as limpid and elegant as Orwell's, Peter Huber is a polymathic genius of our time. With forensic rigor and narrative flair, he banishes from our culture the long-looming incubus of Orwell's *1984* and opens up new horizons of liberation and hope, growing with the promise of the personal computer and the Internet.”
—**George Gilder**

1994 0-02-915335-2 $22.95

MONEY MELTDOWN

Restoring Order to the Global Currency System

Judy Shelton

“Offers a prescient warning and a visionary promise of global monetary and trade reform. Shelton identifies currency chaos as an ominous threat. She issues a clarion call for a new stable international monetary order to serve the needs of an open global economy and embraces the sanctity of sound and honest money as the only logical premise under which free trade and democratic capitalism can succeed around the world.”
—**Jack Kemp**

1994 0-02-929112-7 $24.95

BEFORE THE SHOOTING BEGINS

Searching for Democracy in America's Culture War

James Davison Hunter

“Few contemporary thinkers can match Hunter's ability to join moral urgency with democratic civility. If we are to prevent the culture wars from turning into civil war, people across the political and cultural divides need to understand his analysis and respond to his appeal.”
—**Rev. Richard John Neuhaus,** President, Religion and Public Life

1994 0-02-915501-0 $22.95

NOW IN PAPERBACK—

THE TYRANNY OF THE MAJORITY

Fundamental Fairness in Representative Democracy

Lani Guinier

A MARTIN KESSLER BOOK

1995 0-02-913169-3 $13.00

THE MORAL SENSE

James Q. Wilson

1995 0-02-935406-4 $13.00

DEMOCRACY AND THE PROBLEM OF FREE SPEECH

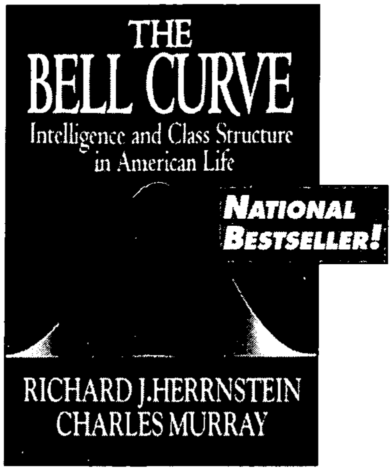
Cass R. Sunstein

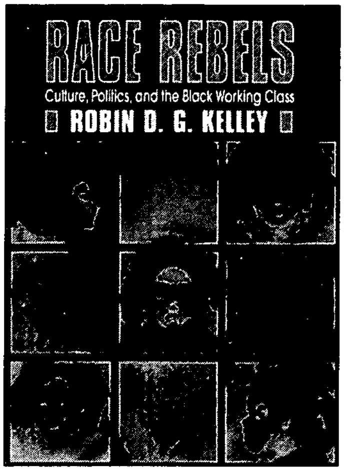
1995 0-02-874000-9 $17.95

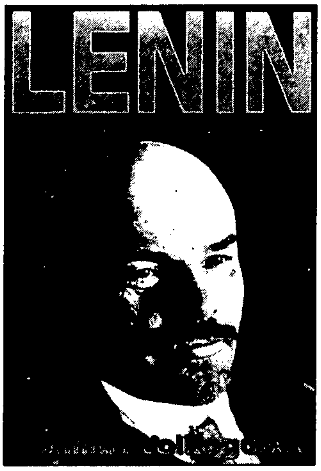
For credit card orders, call toll-free 1-800-223-2336.

Fp THE FREE PRESS
A Division of Simon & Schuster • 866 Third Avenue, New York, NY 10022

NEW FROM THE FREE PRESS







THE BELL CURVE

Intelligence and Class Structure in American Life

Richard J. Herrnstein and Charles Murray

❝Long awaited...massive, meticulous, minutely detailed, clear. Like Darwin's *Origin of Species*—the intellectual event with which it is being seriously compared—*The Bell Curve* offers a new synthesis of research...and a hypothesis of far-reaching explanatory power.❞
—*Forbes Magazine*
1994 0-02-914673-9 $30.00

WHAT WENT WRONG?

The Creation and Collapse of the Black-Jewish Alliance

Murray Friedman

❝The story of Black-Jewish relations has now been told a number of times, but Murray Friedman's account is by far the best informed, the most scholarly, and the most interesting.❞
—**Nathan Glazer,** Harvard University
1995 0-02-910910-8 $24.95

ANCIENT ZIONISM

The Biblical Origins of the National Idea

Avi Erlich

❝Erlich's goal is not scholarly historiography, but rather an original literary excursus into Jewish collective memory. Its brilliance has nothing to do with what might be in the heads of ordinary historians or Biblical scholars or political theorists or Zionist pietists; it is an imaginative catapult. And its mythopoetical power is intellectual, which is why even those who do not accede to its premises may be carried away by its metaphorical daring.❞
—**Cynthia Ozick**
1995 0-02-902352-1 $22.95

RACE REBELS

Culture, Politics, and the Black Working Class

Robin D. G. Kelley

❝*Race Rebels* is African American history at its challenging and transformative best. Kelley's exquisite interweaving of cultural and political dynamics illuminates obscure and unseen sites of Black working-class resistance throughout the 20th century. This is an extraordinary and provocative book.❞
—**Darlene Clark Hine,** author of *The State of Afro-American History: Past, Present & Future*
1994 0-02-916706-X $24.95

JEWS IN THE JAPANESE MIND

The History and Uses of a Cultural Stereotype

David G. Goodman and **Masanori Miyazawa**

❝This dispassionate but provocative volume reveals how Japanese have constructed images of the Jews—both favorable and adverse—since late feudal times. By studying antisemitism and its reverse, philosemitism, Goodman and Miyazawa show that Japanese ideas about Jews stem directly from Japan's modern cultural experience. This unique study is an absorbing essay on relativism and universalism in the contemporary life of the mind in Japan.❞
—**Tom Havens,** University of California, Berkeley
1995 0-02-912482-4 $24.95

For credit card orders,
call toll-free
1-800-223-2336.

LENIN

A New Biography

Dmitri Volkogonov

❝A stunning barrage of new evidence from archives to which few beside Mr. Volkogonov have had, or are likely to have, access...Lenin will still have his champions, but after this book their task will be much more daunting.❞
—***The New York Times Book Review***
1994 0-02-933435-7 $30.00

SUBVERSIVE LAUGHTER

The Liberating Power of Comedy

Ron Jenkins

❝Jenkins' insights come out of a deep affinity with the comic form both as scholar and performer. He never forgets that laughter is humankind's common language. And when he analyzes comedy closer to home, Jenkins displays the kind of social passion and warm humanity that deepens our pleasure in the form.❞
—**Robert Brustein,** Artistic Director, American Repertory Theater
1994 0-02-916405-2 $22.95

THE DOUBTER'S COMPANION

A Dictionary of Aggressive Common Sense

John Ralston Saul

❝At last, an accurate dictionary for our decadent times...Saul has brilliantly subverted the common pieties of commerce and politics with the force of his wit.❞
—**William Grieder,** author of *Who Will Tell the People*
1994 0-02-927722-1 $22.95

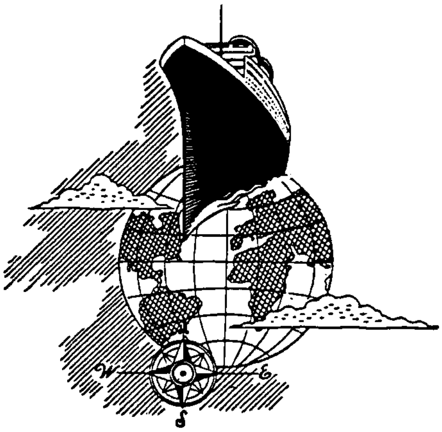
MICHIGAN MICHIGAN MICHIGAN

Kathlyn Taylor Gaubatz

Crime in the Public Mind

". . . Gaubatz has discovered that what the [public] believes about crime and punishment is more complicated than the polling numbers would allow. While clearly in sympathy with those who dissent from get-tough approaches, she offers a balanced, insightful, and timely understanding of public attitudes about criminal justice. Experts, policymakers, students, and concerned citizens should read this fine book." —John J. DiIulio, Jr., Princeton University and Director, Brookings Institution Center for Public Management

cloth $34.50



Paul R. Abramson and Ronald Inglehart

Value Change in Global Perspective

In this pioneering work, Paul R. Abramson and Ronald Inglehart show that the gradual shift from Materialist values (such as the desire for economic and physical security) to Post-materialist values (such as the desire for freedom, self-expression, and the quality of life) is a global phenomenon.

paper $16.95 / cloth $42.50 / April

Nikolaos Zahariadis

Markets, States, and Public Policy

Privatization in Britain and France

A theoretical and empirical examination of the move towards privatization in Britain and France.

cloth $37.50 / April

Victoria E. Marklew

Cash, Crisis, and Corporate Governance

The Rise of National Financial Systems in Industrial Restructuring

Focusing on four large British and French manufacturers during a period of crisis and restructuring, Marklew explores how they found financing. She shows that the way that national financial systems channel money to industry is the key issue in industrial competitiveness.

cloth $39.50 / May

Roy Pierce

Choosing the Chief

Presidential Elections in France and the United States

A thorough analysis of how constitutional frameworks, electoral laws, party systems, social structures, and pivotal historical developments have converged to produce distinctive patterns of presidential politics in two countries.

cloth $39.50

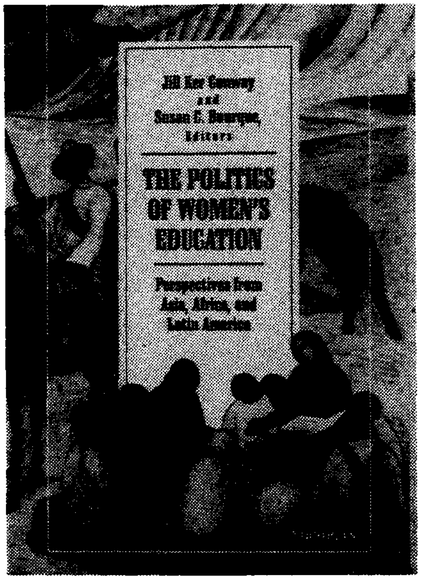
Sidney Fine

"Without Blare of Trumpets"

Walter Drew, the National Erectors' Association, and the Open Shop Movement, 1903–1957

This important study of labor-management strife in the building and construction industry speaks to the role of government in industrial relations.

cloth $54.50



Jill Ker Conway and Susan C. Bourque, Editors

THE POLITICS OF WOMEN'S EDUCATION

Perspectives from Asia, Africa, and Latin America

Now in paper

Jill Ker Conway and Susan C. Bourque, Editors

The Politics of Women's Education

Perspectives from Asia, Africa, and Latin America

Third world women and men discuss efforts to improve the status of women through education.

paper $16.95

William James Adams, Editor

Singular Europe

The Economy and Polity of the European Community after 1992

Scholars from the United States and Europe examine the implications of the fully integrated European market.

paper $22.95

The University of Michigan Press
Dept. VL
Ann Arbor MI 48106-1104

Credit card buyers may fax orders to (800) 876-1922

MICHIGAN

Three superb tools for political science research

HANDBOOK OF STATISTICAL MODELING FOR THE SOCIAL AND BEHAVIORAL SCIENCES

edited by **Gerhard Arminger, Clifford C. Clogg, and Michael E. Sobel**
with a Foreword by **Donald B. Rubin**

Rigorously surveys the most important statistical models and methods used in conducting empirical research in the behavioral and social sciences. Discussions of each model are geared toward the unique problems and data of these disciplines. Self-contained chapters explore key issues that include • difficulties associated with missing data and research based on non-experimental data such as surveys and quasi-experiments • unreliable inferences due to variables measured with error • measurements that are quantitative and continuous at one extreme or categorical and nominal at another • special methods required for studying longitudinal data sets. Includes problems and data for which the model is useful, examples, reviews of technical aspects, and applicable software.

0-306-44805-X/618 pp./ill./1995/$59.50
text adoption price on orders of six or more copies: $34.50 each

RACE AND THE JURY

Racial Disenfranchisement and the Search for Justice

by **Hiroshi Fukurai, Edgar W. Butler, and Richard Krooth**

Named an Outstanding Book for 1994 by The Gustavus Myers Center for the Study of Human Rights in North America. A volume in the Plenum Series in Crime and Justice.

0-306-44144-6/258 pp./ill./1993/$34.95
text adoption price on orders of six or more copies: $24.95 each

POLITICAL BEHAVIOR

Editor: **Richard A. Brody**
Advisory Editor: **Paul M. Sniderman**

An established forum for the interdisciplinary study of individuals, small groups, and large organizations as they interact in and with the political process. Drawing from the fields of economics, psychology, sociology, and political science, *Political Behavior* features cutting-edge theoretical and empirical research essential for students of political behavior.

ISSN: 0190-9320 Subscription: Volume 17, 1995 (4 issues)
Institutional rate: $160.00 in US/$185.00 elsewhere
Personal rate: $42.00 in US/$49.00 elsewhere

Send for a free sample copy!

Book prices are 20% higher outside US & Canada.

THE LANGUAGE OF SCIENCE
Plenum
PUBLISHING CORPORATION

PLENUM PUBLISHING CORPORATION, 233 Spring Street, New York, NY 10013-1578
Telephone orders: 212-620-8000/1-800-221-9369

Moving?......
Moving?......
Moving?...... Don't Forget to Notify APSA

To ensure that all your subscriptions, membership information, and mailings reach you, return this form to APSA at least one month before you move.

Name: ____________________ **Membership Number:** ______________

Work Address:
____
____
____

Home Address:
____
____
____

Phone/Fax: ____________________

E-Mail: ____________________

APSA will mail to your work address unless indicated otherwise. Please mark with an H which items to send to the home address: __ **Renewals** __ **Journals** __ **PSN**

Please return to:

Membership Office, American Political Science Association
1527 New Hampshire Ave., NW, Washington, DC 20036;
or fax : (202) 483-2657

New & Noteworthy from **PENN STATE PRESS**

Closing the Door to Destitution

The Shaping of the Social Security Acts of the United States and New Zealand

RAYMOND RICHARDS

"Raymond Richards adds a comparative dimension to a subject that is often discussed in vague generalities. His study is grounded in 'thick description' of both the New Zealand and American experiences. He has also composed what I believe to be a thoroughly original discussion of the role of insurance companies in the passage of the 1935 Social Security Act. This book should appeal to historians, political scientists, and historical sociologists." —Edward D. Berkowitz, George Washington University

218 pages $42.50

Terrorism in Context

EDITED BY MARTHA CRENSHAW

An interdisciplinary investigation of the phenomenon of terrorism in its political, social, and economic context as it has occurred throughout the world from the nineteenth century to the present. Contributors include Martin A. Miller, Philip Pomper, Donatella della Porta, Peter H. Merkl, Richard Gillespie, David Scott Palmer, Charles Townshend, Paul Wallace, Goldie Shabad and Francisco José Llera Ramo, Ian S. Lustick, Jerrold D. Green, and Michel Wieviorka.

652 pages 18 illus. paper: $25.00

Thomas G. Masaryk

Against the Current, 1882–1914

H. GORDON SKILLING

An analytical study of the ideas and actions of one of Czechoslovakia's leading thinkers and politicians, who served as first president of the new Czech Republic. Skilling's work is unusual in focusing on Masaryk's early career, and it seeks to interpret him in his contemporary proportions and not in terms of his subsequent life as a wartime leader in exile and as President of Czechoslovakia.

264 pages $39.50
For sale in the United States and Canada only

Civil Rights in the United States

EDITED BY HUGH DAVIS GRAHAM

A collection of essays that places the issue of civil rights in America into a historical and comparative perspective. Written from diverse disciplinary, topical, and cultural perspectives, these essays offer readers a broad and historically informed analysis of civil rights policy that should foster reasoned discussion, academic debate, and further research.

166 pages paper: $13.95
Issues in Policy History #4

Religious Routes to Gladstonian Liberalism

The Church Rate Conflict in England and Wales

J. P. ELLENS

"The church rates issue was the great Dissenting issue during the period, and Ellens reveals with insight and admirable clarity the intricacies of the changing relationships as the Liberal party's support waxed and waned." —R. W. Davis, Washington University

316 pages $45.00

A Civil Tongue

Justice, Dialogue, and the Politics of Pluralism

MARK KINGWELL

"Kingwell's central thesis is that civility is both a precondition of justice under conditions of moral pluralism and a commitment that would emerge from a dialogue oriented towards the discovery of just norms. . . . This work makes an important contribution to a vibrant area of contemporary political theory." —J. Donald Moon, Wesleyan University

280 pages paper: $16.95

PENN STATE PRESS
820 N. University Drive
University Park, PA 16802-1003
Fax: (814) 863-1408
MasterCard/VISA orders, call ***800-326-9180***

Harcourt Brace College Publishers
Introduce
A New Era in International Relations Textbooks

World Politics in a New Era
Steven L. Spiegel
University of California, Los Angeles

Written from a post Cold War perspective, *World Politics in a New Era* contains numerous innovative features, including a unique organization that breaks with that of traditional international relations texts. Spiegel's presentation begins with history, proceeds to issues, and concludes with theory.

Paperback 770 pages ISBN 0-03-047574-0

World Politics in a New Era
Steven L. Spiegel

Also New in 1995

American State & Local Politics
Jeffrey M. Stonecash, Syracuse University
Organized around the class/conflict theme of "haves versus have nots," this text provides a unique contemporary approach to the examination of state and local government.
Hardbound 480 pages ISBN 0-15-500368-2

Governance & American Politics: Classic & Contemporary Perspectives
James P. Pfiffner, George Mason University
This supplementary American government reader features a remarkable breadth of outstanding selections from scholars, journalists, and participants in the political process.
Paperback 660 pages ISBN 0-15-501000-X

History of Political Theory, Volume II
George Klosko, University of Virginia
In the second volume of this two-volume set, Klosko offers a clear, reliable analysis of the great ideas and thinkers in the history of political theory. Coverage begins with Machiavelli and proceeds chronologically up to and including Karl Marx.
Paperback 448 pages ISBN 0-03-074014-2

For more information, please contact your local Harcourt Brace Representative or write:

HARCOURT
BRACE

Steve Drummond
Harcourt Brace College Publishers
301 Commerce Street, Suite 3700
Fort Worth, Texas 76102

ELECTRONIC REPRODUCTION PROHIBITED

Kansas

Presidential War Power

Louis Fisher

"This important book deserves the widest possible readership and should be compulsory reading for every Congressman and President. A tour de force." • Leonard W. Levy, editor-in-chief, *Encyclopedia of the American Constitution*. "A lucid and thoughtful work by the nation's top authority on the separation of powers doctrine. Fisher gives a balanced account but pulls no punches in presenting his own views. A classic." • Michael J. Glennon, author of *Constitutional Diplomacy*. "Those who care about the preservation of constitutional government in the United States will want to study this careful, sensible, and deeply disturbing book." • Donald L. Robinson, author of *"To the Best of My Ability."*

288 pages. $29.95 cloth, $14.95 paper

Available at bookstores or from the press
Phone 913-864-4155 Fax 913-864-4586
VISA and MasterCard accepted

The Politicizing Presidency

The White House Personnel Office, 1948–1994

Thomas J. Weko

"This is the most thorough analysis of the institutional presidency and presidential appointments I've seen and will be the definitive study of the White House Personnel Office for some time to come. A superb book." • James Pfiffner, author of *The Modern Presidency*. "A fascinating and much-needed account that sheds important new light on the presidential power of appointment." • Terry M. Moe, author of *The Organization of Interests*.

256 pages. $35.00 cloth, $17.95 paper

Cultivating Congress

Constituents, Issues, and Interests in Agricultural Policymaking

William P. Browne

"An ambitious and excellent book that vividly reflects Browne's remarkable access to members of Congress, their staffs, and other players in the agricultural policy domain. It will be widely read by students, scholars, and activists." • Frank Baumgartner, coauthor of *Agendas and Instability in American Politics*.

216 pages. $29.95 cloth, $14.95 paper

Mr. Social Security

The Life of Wilbur J. Cohen

Edward D. Berkowitz
Foreword by Joseph A. Califano

"A major contribution to modern American political history. Wilbur Cohen's life, as Berkowitz admirably shows, provides a window on the entire process of statebuilding for Social Security in America." • Theda Skocpol, author of *Protecting Soldiers and Mothers*.

416 pages. $34.95

New in Paperback

The Supreme Court and Constitutional Theory, 1953–1993

Ronald Kahn

326 pages. $35.00 cloth, $17.95 paper

Amending America

If We Love the Constitution So Much, Why Do We Keep Trying to Change It?

Richard B. Bernstein
with Jerome Agel

416 pages. $17.95 paper

University Press of Kansas 2501 West 15th Street Lawrence KS 66049

United Nations University Press

New York • Paris • Tokyo

New Series on Multilateralism and the UN System

Global Transformation: Challenges to the State System
Edited by Yoshikazu Sakamoto

The underlying forces that helped to end the Cold War are internationalization and democratization. Both have developed unevenly, giving rise to contradictions, particularly between capitalist internationalization and political democratization. These contradictions pose challenges to the basic framework of the modern international order - the state system. This book shows how the state system, though still strong, is being tested on a global scale.
November 1994, 648pp, Pbk, ISBN 92-808-0855-9, $45.00

State, Society and the UN System: Changing Perspectives on Multilateralism
Edited by Keith Krause and W. Andy Knight

Discusses the relationship between state/society and international institutions to better understand the concept of world order and the role of the United Nations.
December 1994, ISBN 92-808-0885-0, $35.00

The United Nations System: The Policies of Member States
By Chadwick F. Alger, Gene M. Lyons, and John E. Trent

A comparative study of national policies towards the United Nations. Case-studies are given for Algeria, Canada, France, Japan, the Netherlands, Nigeria, the UK, and the US.
March 1995, ISBN 92-808-0884-2, $60.00

UNIPUB
Exclusive U.S. Distributor of UNUP Titles
4611-F Assembly Drive, Lanham, MD 20706
800-274-4888 E-mail: query@kraus.com

• EDUCATION • JOURNALISM • HEALTH • PHYSICAL EDUCATION & RECREATION • ANTHROPOLOGY • HISTORY • HUMANITIES •

POLITICAL SCIENCE • ECONOMICS • JOURNALISM • ART • ANTHROPOLOGY

SOCIOLOGY • PSYCHOLOGY • CRIMINAL JUSTICE • ECONOMICS • HISTORY •

CHOICES SOLUTIONS

B&B CourseKits™

▲ Annual Editions®* ▲ B&B CourseBooks™* ▲ CourseWorks®**
▲ Taking Sides®* ▲ E.L.I.T.E.™ ▲ B&B CourseMedia™ ▲ CourseSupplements™*

Developing New Choices That Offer Innovative Solutions

For more information contact your local Brown & Benchmark representative or call 1-800-338-5371.

Brown & Benchmark
PUBLISHERS

From Wm. C. Brown Communications, Inc. / A Times Mirror Company
25 Kessel Court • Madison, Wisconsin 53711
*Includes materials formerly published by the The Dushkin Publishing Group, Inc. **A service formerly provided by Kinko's.

• PHILOSOPHY • EDUCATION • SPEECH COMMUNICATIONS • JOURNALISM • HEALTH • MUSIC • HUMANITIES • SOCIOLOGY •

Kent

THE SALMON P. CHASE PAPERS

Volume 2: Correspondence, 1823–1857

John Niven, ed.; James P. McClure, sr. assoc. ed.;
Leigh Johnsen, assoc. ed.; Steve Leikin, asst. ed.;
William M. Ferraro, asst. ed.

Transcriptions of Salmon P. Chase's most important letters to the prominent political figures of his day, including Martin and John Van Buren, Gamaliel Bailey, Frederick Douglass, Joshua Giddings, John P. Hale, William H. Seward, and Charles Sumner, are among those collected in this first-ever published edition of his correspondence.

$35 cloth, 520 pp., illus.

THE LIBYAN ARENA

The United States, Britain, and the Council of Foreign Ministers, 1945–1948

Scott L. Bills

"Scott L. Bills illuminates a crucial, yet sorely neglected aspect of cold war diplomacy. His sensitive, incisive, and balanced account of the conflicts engendered by the disposition of Italy's former African colonies after World War II not only makes an important contribution to the history of the cold war, but deepens as well our understanding of the roots of Western-Third World tensions."—Robert J. McMahon

$30 cloth, 232 pp., illus.

OHIO POLITICS

Edited by Alexander P. Lamis
With the assistance of Mary Anne Sharkey

A team of veteran journalists and political scientists analyzes in depth the major political events in Ohio since 1944 to produce the first comprehensive survey of the state's post-World War II politics.

$35 cloth, $17 paper, 452 pp., illus.

THE PURSUIT OF PUBLIC POWER

Political Culture in Ohio, 1787–1861

Edited by Jeffrey P. Brown & Andrew R. L. Cayton

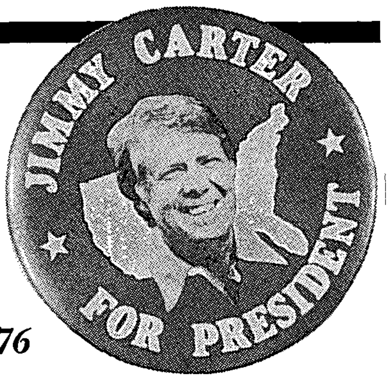
Twelve essays by noted historians explore the origins and nature of political culture in Ohio from the American Revolution until the Civil War. Topics include voting practices, the role of the state in national economic development, and the relationship between religion and politics.

$22 paper, 272 pp.

AVAILABLE AT BOOKSTORES OR FROM

THE KENT STATE UNIVERSITY PRESS
P. O. Box 5190 • Kent, OH 44242-0001
1-800-247-6553, ext. 188

Electing Jimmy Carter



The Campaign of 1976

Patrick Anderson

"The most evocative political memoir in years. . . . Anderson's book should be consulted for its full-blooded portrait of this astonishingly complex and contradictory man. More broadly, [it] will hold a fascination for anyone interested in the way words shape politics, and vice versa."

—*Washington Post Book World*

$24.95

Eisenhower

A Centenary Assessment

Edited by Günter Bischof and Stephen E. Ambrose

"A new New Look at Eisenhower by some of the liveliest minds in the historical profession. A truly wonderful collection!"

—Ernest R. May
Charles Warren Professor of History
Harvard University

$30.00

Fathers of International Thought

The Legacy of Political Theory

Kenneth W. Thompson

Renowned foreign affairs scholar Kenneth W. Thompson returns to the writings of sixteen early thinkers in order to better understand the issues and problems that recurrently beset global politics. He shows how the ideas of "the fathers" have application to the current international scene and how decision makers can avoid repeating less successful patterns in foreign policy by studying those classics.

$27.50 cloth, $9.95 paper

Louisiana State University Press
P. O. Box 25053 • Baton Rouge, LA 70894-5053
Credit-card orders: 800-861-3477

WESTVIEWPRESS

Transforming American Politics
Lawrence C. Dodd
series editor

Congressional Politics
The Evolving Legislative System
Second Edition
Leroy N. Rieselbach
481 pages • $65.00 hc • $29.95 pb

Campaigns and Elections American Style
edited by James A. Thurber and Candice J. Nelson
244 pages • $55.00 hc • $19.95 pb

The New American Politics
Reflections on Political Change and the Clinton Administration
edited by Bryan D. Jones
April 1995 • 347 pages • $55.00 hc • $24.95 pb

New Texts Now Available
With Coverage of the 1994 Elections

City-Building in America
Anthony M. Orum
250 pages • $65.00 hc • $19.95 pb

The Dependent City Revisited
The Political Economy of Urban Development and Social Policy
Paul Kantor
262 pages • $65.00 hc • $19.95 pb

The Los Angeles Riots
Lessons for the Urban Future
edited by Mark Baldassare
255 pages • $49.95 hc • $16.95 pb

Unequal Struggle
Class, Gender, Race, and Power in the U.S. Congress
John C. Berg
187 pages • $59.00 hc • $19.95 pb

Nations in Turmoil
Conflict and Cooperation in Eastern Europe
Second Edition
Janusz Bugajski
240 pages • $54.95 hc • $19.95 pb

Whose World Order?
Uneven Globalization and the End of the Cold War
edited by Hans-Henrik Holm and Georg Sørensen
236 pages • $45.00 hc • $17.95 pb

Mercy Under Fire
War and the Global Humanitarian Community
Larry Minear and Thomas G. Weiss
April 1995 • 224 pages • $55.00 hc • $17.95 pb

Russian Foreign Policy Since 1990
edited by Peter Shearman
325 pages • $55.00 hc • $19.95 pb

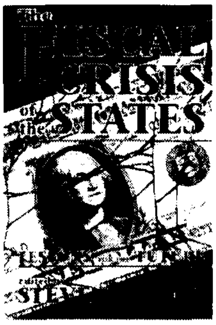
National Security Directives of the Reagan and Bush Administrations
The Declassified History of U.S. Political and Military Policy, 1981–1993
Christopher Simpson
1002 pages • $95.00 hc

The European Sisyphus
Essays on Europe, 1964–1994
Stanley Hoffmann
336 pages • $69.95 hc • $19.95 pb

5500 Central Avenue • Boulder, Colorado • 80301-2877
Phone (303) 444-3541 • Fax (303) 449-3356 • **1-800-456-1995** • MasterCard and VISA accepted

GEORGETOWN

New Books on Public Policy



The Fiscal Crisis of the States

Lessons for the Future

STEVEN D. GOLD, Editor

"As the nation debates a balanced-budget amendment for the federal government, the experience of states in managing their fiscal affairs becomes more and more important. The picture has been clearly drawn by Steven Gold and his colleagues in this book. It is a valuable contribution to an important discussion."—David S. Broder, *The Washington Post* *320 pp. May 1995*
ISBN 0-87840-574-7, cloth $42.50 ISBN 0-87840-575-5, paper, $19.95

The Founders, the Constitution, and Public Administration

A Conflict in World Views

MICHAEL W. SPICER

"Spicer has posed an elegant intellectual dichotomy between the assumptions of the Constitution and the implicit values of American public administration...The argument is provocative, fresh...on the cutting edge of public administration theory."—Professor Charles Goodsell, Center for Public Administration and Policy, Virginia Polytechnic Institute and State University *128 pp. April 1995*
ISBN 0-87840-581-X, cloth, $27.50 ISBN 0-87840-582-8, paper, $13.95

Announcing a New Textbook—

Ethics and International Affairs

A Reader

Joel H. Rosenthal, Editor

This collection of some of the best contemporary scholarship in ethics and international affairs explores the connection between moral traditions and decision making. It offers organizing principles for the study of the field.

Contributors: Joel H. Rosenthal * Robert J. Myers * Stanley Hoffmann * Jacques Barzun * Alberto R. Coll * Cho-yun Hsu * James Turner Johnson * Charles W. Kegley, Jr. * Terry Nardin * Sissela Bok * Charles R. Beitz * Augustus Richard Norton * David Hendrickson * Thomas G. Weiss * Jack Donnelly * Robert E. Goodin * Robert L. Phillips * Mick Moore * Mark Robinson.
314 pp. March 1995
ISBN 0-87840-578-X, paper, $18.95

Playing By The Rules

American Trade Power and Diplomacy in the Pacific

Michael P. Ryan

Ryan evaluates the nature and effectiveness of U.S. trade diplomacy with Japan, Korea, Taiwan, and China in the 1970s and 1980s by examining the diplomatic strategies used by the U.S. Trade Representative to enforce Section 301 of the 1974 Trade Act.
320 pp. July 1995 ISBN 0-87840-579-8, cloth, $42.50

Peacemaking

Moral and Policy Challenges for a New World

Gerard F. Powers, Drew Christiansen, S.J.,
and Robert T. Hennemeyer, Editors

"Written from varying ideological perspectives, but united in a concern for the application of moral values to international relations, this is a provocative and timely contribution to debate about the current and future course of foreign policy."—Paul E. Sigmund, Professor of Politics, Princeton University
Distributed for the United States Catholic Conference
368 pp. January 1995 ISBN 1-55586-682-4, paper, $19.95

GEORGETOWN UNIVERSITY PRESS

P.O. Box 4866 Hampden Station, Baltimore, MD 21211-4866
Phone: 410-516-6995 FAX: 410-516-6998

Forthcoming in the

PITT SERIES IN POLICY AND INSTITUTIONAL STUDIES

Bert A. Rockman, Series Editor

Post-Passage Politics

Bicameral Resolution in Congress

Stephen D. Van Beek

Explores and explains how the House and Senate reach agreement on major public policy issues. The focus is both on the processes used to resolve disagreements and on the various individuals playing roles in the decision making.

June 1995 / $49.95 cloth (tentative)

Fragile Democracies

Legacies of Authoritarian Rule

Gretchen Casper

"Casper's emphasis on social institutions as a focal point for understanding the legacy of authoritarianism makes this study distinctive." —David Pion-Berlin, University of California, Riverside

August 1995 / $19.95 paper (tentative)

Global Competitiveness and Industrial Growth in Taiwan and the Philippines

Cheng-Tian Kuo

"The case studies, especially, are historically informed, rich in descriptions, and pregnant with theoretical implications." —Steve Chan. Case studies of major Taiwanese and Philippine industries prove that close business-government cooperation under state corporatism provides the best opportunity for a developing nation to industrialize successfully.

May 1995 / $49.95 cloth (tentative)

Neighborhood and Nation in Tokyo, 1905-1937

Sally Ann Hastings

How local groups and national policies combined to shape Japan's modern political community.

May 1995 / $59.95 cloth (tentative)

Regulation in the Reagan-Bush Era

The Eruption of Presidential Influence

Barry D. Friedman

"I have yet to see a work that goes about explaining this process in such depth and clarity." —Daniel P. Franklin, Georgia State University

June 1995 / $59.95 cloth (tentative)

Recently published

Scrambling for Protection
The New Media and the First Amendment
Patrick Garry

Making Common Sense of Japan
Steven R. Reed

Thatcher, Reagan, Mulroney
In Search of a New Bureaucracy
Donald J. Savoie

The Japanese Prime Minister and Public Policy
Kenji Hayao

Researching the Presidency
Vital Questions, New Approaches
George C. Edwards, III, John H. Kessel, and Bert A. Rockman, eds.

Careers in City Politics
The Case for Urban Democracy
Timothy Bledsoe

The State Roots of National Politics
Congress and the Tax Agenda, 1978-1986
Michael B. Berkman

The Political Psychology of the Gulf War
Leaders, Publics, and the Process of Conflict
Stanley A. Renshon, ed.

Available at bookstores, or

University of Pittsburgh Press

C/O CUP SERVICES, P.O. BOX 6525, ITHACA, NY 14851 / 800-666-2211

American Hegemony

Political Morality in a One-Superpower World

Lea Brilmayer

Is it morally acceptable for one nation to police the world? Are there circumstances under which one nation has either the right or the obligation to dominate the others? This "fascinating and well-written" (Robert O. Keohane) book is the first to examine these and other moral issues raised by America's status as the sole world superpower after the end of the Cold War. $30.00

The Fading of the Greens

The Decline of Environmental Politics in the West

Anna Bramwell

Although the green movement has had a major impact on public awareness and concern for environmental issues, green political parties in Europe and the United States have not won elections. In this controversial look at the development of green parties and ideology since World War II, an environmental expert and policymaker explores why the greens have failed to create a new politics. $25.00

Challengers, Competition, and Reelection

Comparing Senate and House Elections

Jonathan S. Krasno

"Krasno deals with the question of why Senate elections are more competitive than House elections better than anyone else. His book will be regarded as the authoritative treatment of the subject."—David Mayhew $26.00

The Laws of War

Constraints on Warfare in the Western World

Edited by Michael Howard, George J. Andreopoulos, and Mark R. Shulman

This book explores not only the formal constraints on the conduct of war throughout Western history but also the unwritten conventions about what is permissible in the course of military operations. $30.00

To order call
1-800-YUP-READ

Yale University Press
P.O. Box 209040
New Haven, CT 06520

The Losing Parties

Out-Party National Committees, 1956-1993

Philip A. Klinkner

"An important and distinctive contribution to the study of party politics. The case studies of national committees are rich and informative and the writing style is both clear and readable. Drawing on party documents, interviews with party officials, and contemporary accounts, Klinkner provides a fascinating account of opposition party politics."—Leon D. Epstein $30.00

Group Psychology and Political Theory

C. Fred Alford

"Alford develops a political theory based on the study of small group dynamics, focusing on the tensions within each of us between individuality and group membership."—Edward R. Shapiro $25.00

The Green Rainbow

Environmental Groups in Western Europe

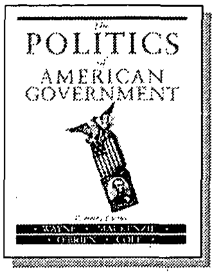
Russell J. Dalton

"*The Green Rainbow* makes a major advance in explaining the rise of the environmentalist movement, and in clarifying the motivation and strategies underlying contemporary social movements more broadly."—Ronald Inglehart Illus. $35.00

ST. MARTIN'S PRESS

JUST PUBLISHED

AMERICAN POLITICS



THE POLITICS OF AMERICAN GOVERNMENT

Complete and Basic Editions

STEPHEN J. WAYNE, *Georgetown University* ■ G. CALVIN MACKENZIE, *Colby College* ■ DAVID M. O'BRIEN, *University of Virginia* ■ RICHARD L. COLE, *University of Texas at Arlington*

Takes students inside government to examine how the struggle for power shapes the processes, institutions, and policy decisions of American government ■ Demonstrates how individuals and groups use the political system to advance their own values and interests ■ Helps students understand how politics influences government, and how government, in turn, influences politics

Complete Edition: 1995 / 864 pp. / Cloth / ISBN 0-312-06751-8
Basic Edition (without policy chapters): 1995 / 704 pp. / Paper / ISBN 0-312-11168-1
Full ancillary package available
Available Spring 1995: Free Booklet with results and a complete analysis of 1994 elections

NEW

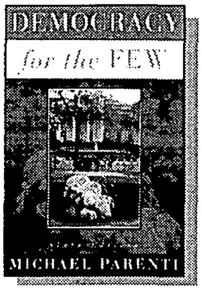
Ralph Nader Presents, PRACTICING DEMOCRACY

A Guide to Student Action

KATHERINE ISAAC, *Center for Study of Responsive Law*

Serves as a guide to the strategies that students can use to become more active participants in the political system to advance the causes that are most important to them ■ Includes a bibliography and list of citizen activist groups ■ Encourages students to become politically active for the betterment of themselves and their communities

1995 / 112 pp. / Paper / ISBN 0-312-10789-7
Also offered as a free supplement to adopters of *The Politics of American Government*



NEW SIXTH EDITION

DEMOCRACY FOR THE FEW

MICHAEL PARENTI

A compelling and well-documented critique of American government ■ Examines the ways in which government serves the interests of corporations, the military-industrial complex, and wealthy citizens ■ Contains new material on crime, corporate economic crisis, and recession; an updated media chapter; and criticisms of the Clinton administration

1995 / 354 pp. / Paper / ISBN 0-312-05233-2

NEW FIFTH EDITION

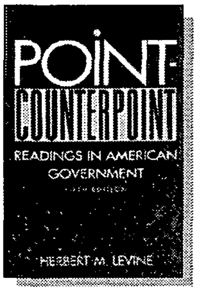
POINT-COUNTERPOINT

Readings in American Government

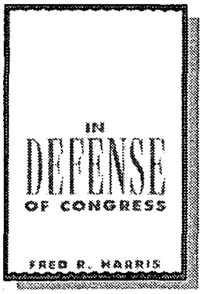
HERBERT M. LEVINE

Features 42 pro/con readings that engage students in debate about 21 significant issues in American politics, including government regulation of pornography, freedom of the press during wartime, affirmative action, and the durability of the federal system ■ Essays and topics chosen for their accessibility for beginning undergraduate students ■ 75% of the reprinted readings are new to the fifth edition; two readings have been heavily revised

1995 / 390 pp. / Paper / ISBN 0-312-09968-1
Instructor's Manual available



NEW



IN DEFENSE OF CONGRESS

FRED R. HARRIS, *University of New Mexico*

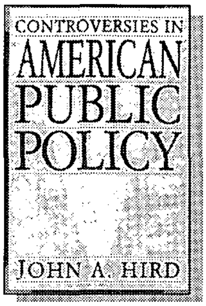
Offers a passionate argument on behalf of Congress, along with suggestions for improving the institution ■ Demonstrates how public discourse has distorted the roles assigned to Congress and the President in the crafting of public policy ■ Gives careful consideration to proposals for reform in legislative ethics and accountability while trying to present a more positive image of Congress

1995 / 177 pp. / Paper / ISBN 0-312-09456-6

SMP St. Martin's Press • *College Division*
175 Fifth Avenue, New York, NY 10010
1-800-446-8923
Fax:(212)780-0115

FROM ST. MARTIN'S PRESS

NEW

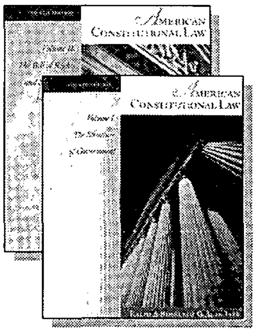


CONTROVERSIES IN AMERICAN PUBLIC POLICY

JOHN A. HIRD, *University of Massachusetts at Amherst*

A pro/con reader containing 36 readings that debate 18 contemporary issues in public policy ■ Covering such topical issues as national health insurance, AIDS policy, abortion, gun control, and environmental policy, the book is designed to complement any core text in public policy, whether organized around process or problems

1995 / 416 pp. / Paper / ISBN 0-312-10407-3



NEW FOURTH EDITION

AMERICAN CONSTITUTIONAL LAW

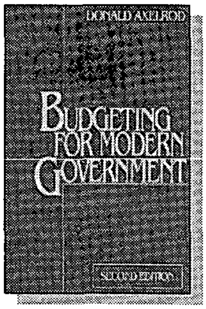
Volume I: The Structure of Government
Volume II: The Bill of Rights and Subsequent Amendments

RALPH A. ROSSUM, *University of Redlands*
G. ALAN TARR, *Rutgers University, Camden*

A comprehensive introductory text for courses in constitutional law and civil liberties ■ The text is organized around 3 premises – that the principles of the Constitution can best be understood by grappling with alternative positions on constitutional controversies; that no interpretation of the constitution can be evaluated properly without an appreciation of what those who initially drafted and ratified the constitution sought to accomplish; and that to focus exclusively on the judiciary is to ignore the important contributions of other government institutions ■ Approximately 20% of the 172 cases are new to this edition

February 1995
Volume I: 512 pp. / Paper / ISBN 0-312-10260-7
Volume II: 656 pp. / Paper / ISBN 0-312-10261-5
Annual Supplement available

PUBLIC ADMINISTRATION



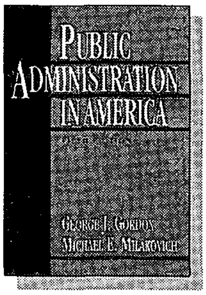
NEW SECOND EDITION

BUDGETING FOR MODERN GOVERNMENT

DONALD AXELROD, *State University of New York at Albany*

A comprehensive and up-to-the-minute examination of the issues, problems and processes of budgeting at all levels of government ■ Emphasis on budgeting as a political process ■ Includes a new chapter on "off-budget" budgeting ■ Includes many useful examples and illustrations

1995 / 421 pp. / Cloth / ISBN 0-312-08417-X



NEW FIFTH EDITION

PUBLIC ADMINISTRATION IN AMERICA

GEORGE J. GORDON, *Illinois State University*
MICHAEL E. MILAKOVICH, *University of Miami*

Integrates political and managerial perspectives on public administration ■ Places current topics and issues in their conceptual, historical, and administrative contexts ■ New edition features new or expanded information on privatization, total quality management, and the movement to "reinvent government"

1995 / 544 pp. / Cloth / ISBN 0-312-08970-8
Instructor's Manual available

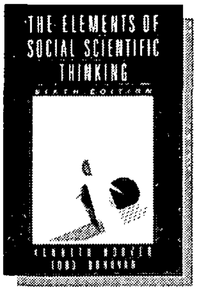
SMP St. Martin's Press • *College Division*
175 Fifth Avenue, New York, NY 10010
1-800-446-8923
Fax:(212)780-0115

ST. MARTIN'S PRESS

ST. MARTIN'S PRESS

JUST PUBLISHED

RESEARCH METHODS



NEW SIXTH EDITION

THE ELEMENTS OF SOCIAL SCIENTIFIC THINKING

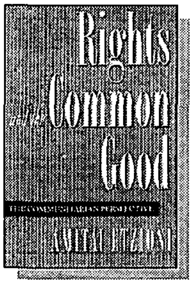
KENNETH HOOVER *and* TODD DONOVAN, *Western Washington University*

A basic introduction to the vocabulary, concepts, and objectives of social scientific thought ■ Readable and engaging style provides students with a common-sense, rather than technical, approach to grasping major concepts ■ Accessible to students with no prior statistical background ■ Cross-disciplinary examples will interest students from a wide variety of areas in the social sciences

1995 / 224 pp. / Paper / ISBN 0-312-09643-7

POLITICAL THEORY/PUBLIC POLICY

NEW



RIGHTS AND THE COMMON GOOD

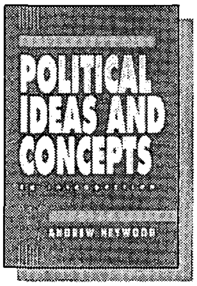
The Communitarian Perspective

AMITAI ETZIONI, *The George Washington University*

Outlines the communitarian perspective on significant public policy issues ■ Provides examples of what communitarian thinking means for politics, culture, and economics ■ Articles and case studies encourage students to think across interdisciplinary lines and draw their own conclusions on issues ■ Examines the foundations as well as the contemporary focus of communitarian thought and provides a critical analysis of individual rights and responsibilities

1995 / 293 pp. / Paper / ISBN 0-312-08968-6

NEW



POLITICAL IDEAS AND CONCEPTS:

An Introduction

ANDREW HEYWOOD, *Orpington College*

Introduces students to the major concepts used in political science ■ Written for beginning students in political science, comparative politics, and political theory, the book explores terms and concepts such as government, the state, the nation, power, equality, and revolution ■ Includes brief boxes that explore the contributions of important political "thinkers" such as Thomas Aquinas, Hannah Arendt, Karl Marx, and Mary Wollstonecraft

1995 / 369 pp. / Paper / ISBN 0-312-11220-3

COMPARATIVE POLITICS

NEW

THE POLITICS OF CHANGE

The Transformation of the Former Soviet Union

CAROL BARNER-BARRY *and* CYNTHIA HODY, *University of Maryland, Baltimore County*

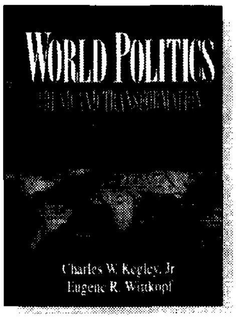
Explores the political, economic, and cultural issues involved in the disintegration of the Soviet Union and the emergence of newly independent states ■ Examines Russia and other post-Soviet states as they attempt to develop their own identities as 15 separate democracies, as they try to develop their own types of market economies, and as they try to integrate themselves into an international system that is, itself, in flux ■ Focuses on the causes and meaning of change, thereby raising more general issues about the process of political transition

1995 / 432 pp. / Paper / ISBN 0-312-09079-X

SMP St. Martin's Press • *College Division*
175 Fifth Avenue, New York, NY 10010
1-800-446-8923
Fax:(212)780-0115

FROM ST. MARTIN'S PRESS

INTERNATIONAL RELATIONS/AMERICAN FOREIGN POLICY



NEW FIFTH EDITION

WORLD POLITICS

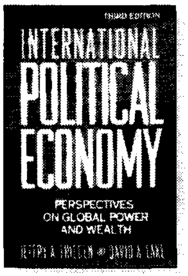
Trend and Transformation

CHARLES W. KEGLEY, JR., *University of South Carolina*
EUGENE R. WITTKOPF, *Louisiana State University*

Examines the key actors and policy issues in international relations ■ By providing historical and analytical detail, the book examines how emerging trends are likely to influence global politics over the coming years ■ New edition includes a more thorough analysis of international relations in the post-Cold War, multipolar world ■ Sections on theory have been updated and include discussions on neoliberalism and feminism ■ Includes increased coverage of population issues, ethics, democratization, ethnic nationalism, the cultural dimensions of world politics, and the regionalization of the world political economy

1995 / 661 pp. / Paper / ISBN 0-312-10658-0
Instructor's Manual and Computerized Test Item File available

NEW


THIRD EDITION
INTERNATIONAL POLITICAL ECONOMY
PERSPECTIVES ON GLOBAL POWER AND WEALTH

NEW THIRD EDITION

INTERNATIONAL POLITICAL ECONOMY

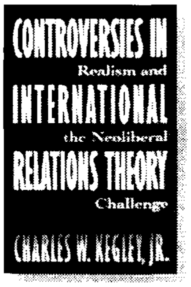
Perspectives on Global Power and Wealth

JEFFRY A. FRIEDEN, *University of California, Los Angeles*
DAVID A. LAKE, *University of California, San Diego*

Offers readings by leading economists and political scientists on perspectives, history, and problems in the global political economy ■ Two-thirds of the articles are new ■ Includes a new section on developing and transitional economies ■ Provides greater attention to the tensions between domestic and international and to state and societal explanations of international political economy

1995 / 546 pp. / Paper / ISBN 0-312-08418-8

NEW


CONTROVERSIES IN INTERNATIONAL RELATIONS THEORY
Realism and the Neoliberal Challenge
CHARLES W. KEGLEY, JR.

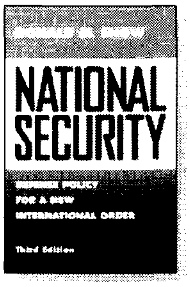
CONTROVERSIES IN INTERNATIONAL RELATIONS THEORY

Realism and the Neoliberal Challenge

CHARLES W. KEGLEY, JR., *University of South Carolina*

Presents 14 essays (9 original, 2 heavily revised, and 2 reprints) on the debate between the two dominant theories in contemporary international relations – realism and neoliberalism ■ Written primarily for a student audience, a distinguished set of contributors identify the assumptions that underlie the two perspectives and assess the ability of the theories to help students and policy-makers understand the character of contemporary world affairs

1995 / 374 pp. / Paper / ISBN 0-312-09653-4



NEW THIRD EDITION

NATIONAL SECURITY

Defense Policy for a New International Order

DONALD M. SNOW, *University of Alabama*

Explores the concept of national security, the policy- and decision-making process in national security, and the strategic use of conventional and nuclear arms ■ New edition focuses on the challenges the U.S. faces when dealing with new patterns of unrest and violence in the post-Cold War world ■ In considering U.S. security issues, this edition presents a two-tier framework that distinguishes between major industrial democracies and the rest of the world, which includes developing nations and authoritarian regimes

1995 / 336 pp. / Paper / ISBN 0-312-10595-9

SMP St. Martin's Press • *College Division*
175 Fifth Avenue, New York, NY 10010
1-800-446-8923
Fax:(212)780-0115

ST. MARTIN'S PRESS

COMING THIS SPRING

Passions and Constraint

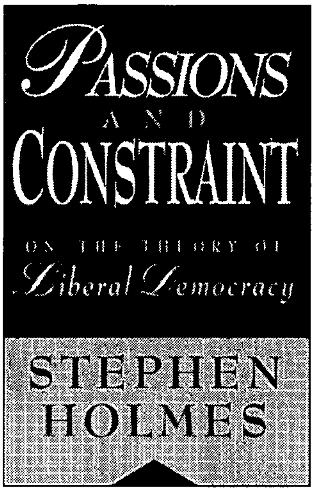
On the Theory of Liberal Democracy

Stephen Holmes

In this collection of essays on the core values of liberalism, Holmes challenges commonly held assumptions about liberal theory.

"This is a brilliantly focused defense of liberalism. Indeed, it will quickly become the 'manifesto' for what is a rapidly growing movement to reclaim liberalism as a truly progressive and humane ideology after decades of political and academic assault."—Issac Kramnick, Cornell University

Cloth $29.95 360 pages



Actively Seeking Work?

The Politics of Unemployment and Welfare Policy in the United States and Great Britain

Desmond King

"This compelling work will become a standard reference on the development of labor market policy in the U.K. and the U.S."—John D. Stephens, University of North Carolina, Chapel Hill

***Paper $17.95 352 pages illus.**

Political Style

The Artistry of Power

Robert Hariman

Hariman demonstrates how matters of style influence politics. He looks at effective political artistry in figures from antiquity to modern politicians such as Vaclav Havel, Ronald Reagan, and Bill Clinton.

***Paper $14.95 248 pages**

Securing Religious Liberty

Principles for Judicial Interpretation of the Religion Clauses

Jesse H. Choper

In this book, a leading constitutional scholar proposes a set of guidelines meant to provide for the consistent application of the First Amendment's Religion Clauses. Choper's guidelines are designed to provide maximum protection for religious freedom without granting anyone an advantage, inflicting a disadvantage, or causing an unfair burden.

Cloth $24.95 208 pages

Cross-Level Inference

Christopher H. Achen

An indispensable guide and reference to a critical issue for quantitative researchers in political science and policy analysis, history, sociology, and geography, this volume explains why older methods like ecological regression so often fail, and gives the most comprehensive treatment available of the promising new techniques for cross-level inference.

***Paper $18.95 256 pages illus.**

From Art to Politics

How Artistic Creations Shape Political Conceptions

Murray Edelman

In this book, Edelman continues his quest to understand the influence of perception on the political process by turning to the role of art. He argues that art provides us with models, scenarios, narratives, and images we draw upon in order to make sense of political events. His exploration illuminates the different ways art shapes political perceptions and actions to promote as well as inhibit diversity and democracy.

Cloth $18.95 162 pages illus.

Now in Paper

Bitter Fruit

Black Politics and the Chicago Machine, 1931-1991

William J. Grimshaw

"Writing at a time when black dominant coalitions have been overturned in New York, Los Angeles and Philadelphia, Grimshaw provides a timely and richly documented account of how black politics, Chicago style, though it participated in the rise of the Daley machine and was responsible for its fall, was unable to institutionalize a reform tradition to succeed the machine era."—David H. Tabb, *American Political Science Review*

Paper $12.95 254 pages

Fear of Diversity

The Birth of Political Science in Ancient Greek Thought

Arlene W. Saxonhouse

"[This book] could be instrumental in bringing to the current diversity debate a very desirable dignity, a dignity commensurate with this complex and troubling and *ancient* human issue."—Eva T. H. Brann, *Review of Politics*

Paper $14.95 268 pages

The Promise of Pragmatism

Modernism and the Crisis of Knowledge and Authority

John Patrick Diggins

"Diggins's superbly informed book comprises a comprehensive history of American pragmatic thought. . . . [A] richly intelligent book."—Mark Edmundson, *Washington Post Book World*

Paper $18.95 528 pages

**Cloth edition available*

CHICAGO **The University of Chicago Press**
5801 South Ellis Avenue, Chicago, Illinois 60637

Johns Hopkins Goes to Washington

With Reverence and Contempt

How Americans Think about Their President

THOMAS S. LANGSTON

"Are individual presidents at fault for a series of failed presidencies—or are the expectations of the American people to blame? Thomas Langston's fascinating analysis of the dysfunctional relationship between Americans and their presidents is required reading for anyone concerned with the future of American politics."—Michael Lind, Senior Editor, *Harper's Magazine*

$24.95 hardcover

Secret Affairs

Franklin Roosevelt, Cordell Hull, and Sumner Welles

IRWIN F. GELLMAN

"Gellman's findings corroborate the impressions I have formed in my own work on the formation of foreign policy in the Roosevelt years. *Secret Affairs* is not only an important contribution to the history of American foreign policy, but it is a good story, splendidly researched and well told."
—Arthur Schlesinger, Jr.

$29.95 hardcover

The New Politics of Public Policy

EDITED BY MARC K. LANDY AND MARTIN A. LEVIN

A group of leading experts challenge the view of the Bush-Reagan era as one characterized by policy gridlock. Contributors include James Q. Wilson, Aaron Wildavsky, Peter Schuck, Martin Shapiro, Wilson C. McWilliams, Robert Kagan, R. Shep Melnick, James Morone, Timothy Conlan, David Beam, Margaret Wrightson, Marc Landy, Martin Levin, and Paul Quirk.

$16.95 paperback

Legislators, Leaders, and Lawmaking

The U.S. House of Representatives in the Postreform Era

BARBARA SINCLAIR

Sinclair traces the emergence during the 1980s of a House majority party leadership that is highly active and deeply involved in multiple aspects of the legislative process, but one that leads by means of a participatory and inclusive style.

$39.95 hardcover

Partners in Public Service

Government-Nonprofit Relations in the Modern Welfare State

LESTER M. SALAMON

Lester Salamon pioneered the study of nonprofit organizations and of their cooperation with government in the development and delivery of important social and economic services. This book brings together some of his most important work on the changing relationship between government and the voluntary sector.

$16.95 paperback

Cityscapes and Capital

The Politics of Urban Development

MICHAEL A. PAGANO AND ANN O'M. BOWMAN

"Pagano and Bowman link urban leadership, vision and image, and detailed analysis of the substantive policies used to promote local economic development. They set up interesting, broad themes, but also present detailed evidence from real cities."—Dennis R. Judd, University of Missouri-St. Louis

$32.50 hardcover

The Amendments to the Constitution

A Commentary

GEORGE ANASTAPLO

A companion to the widely acclaimed *The Constitution of 1787*, this new book by eminent constitutional scholar George Anastaplo examines the nature and effects of the twenty-seven amendments to the U.S. Constitution. For Anastaplo, these amendments implement the equality, liberty, and rule of law principles that are fundamental to the American system of government.

$16.95 paperback

Baltimore Unbound

A Strategy for Regional Renewal

DAVID RUSK

In his highly acclaimed book, *Cities without Suburbs*, David Rusk explained why regions with wealthy suburbs surrounding a poor central city face continuing economic hardship. Now he applies his ideas in an illuminating study of Baltimore's continuing economic stagnation.

Distributed for the Abell Foundation

$14.95 paperback

On the Limits of the Law

The Ironic Legacy of Title VI of the 1964 Civil Rights Act

STEPHEN C. HALPERN

"Stephen Halpern has made a substantial and original contribution to the analysis of law and civil rights. Concentrating on original or primary sources and including very informative interviews, he offers a superb review of the historical and political context of the Civil Rights Act of 1964 and the United States Supreme Court's desegregation decisions. All who are interested in civil rights history and enforcement, the administrative process, and the role of courts in pursuing racial and social justice will want to read this book."—Kenneth Tollett, Howard University

$18.95 paperback

Water Resources Management

In Search of an Environmental Ethic

DAVID LEWIS FELDMAN

"In one slim volume, Feldman has managed to combine a history of U.S. water policy, two in-depth case studies on the politics of water, an analysis of the institutional biases affecting U.S. water policy, and a discussion of water policy in France."
—*Journal of Politics*

$16.95 paperback

The Johns Hopkins University Press
Hampden Station, Baltimore, Maryland 21211
To order, call 1-800-537-5487.

NEW FROM

BERGIN & GARVEY GREENWOOD PRESS PRAEGER

Women in World Politics
An Introduction
Edited by Francine D'Amico and Peter R. Beckman

Where are the women in world politics? What has their impact been? What difference does gender make in world leadership? Interviews and autobiographies of important political women leaders are used to answer these questions.

Bergin & Garvey. 1995. 248 pages. 0-89789-410-3. $59.95

J. William Fulbright
A Bibliography
Compiled by Betty Austin

The first annotated bibliography to assess the literature on Senator J. William Fulbright, his career and role in foreign relations, and the Fulbright educational exchange program.

Greenwood Press. 1995. 216 pages. 0-313-26336-1. $65.00

In Sight of Surrender
The U.S. Sanctions Campaign
Against South Africa, 1946-1993
By Les de Villiers

Easy-to-read thought-provoking insider history showing how U.S. sanctions against South Africa brought about radical political change there.

Praeger Publishers. 1995. 248 pages. 0-275-94982-6. $55.00

U.S.-Latin American Policymaking
A Reference Handbook
Edited by David W. Dent

An authoritative, systematic, single-volume sourcebook of who makes foreign policy, how it is made, and what U.S. policy has been since the 1960s.

Greenwood Press. 1995. 592 pages. 0-313-27951-9. $95.00

Environmental Policies in the Third World
A Comparative Analysis
Edited by O. P. Dwivedi and Dhirendra K. Vajpeyi

Environmental Policies in the Third World analyzes environmental problems and policies in developing countries and new prospects for international cooperation

Greenwood Press. 1995. 240 pages. 0-313-29397-X. $59.95

Southern State Party Organizations and Activists
By Charles D. Hadley and Lewis Bowman

This groundbreaking volume describes and analyzes grassroots political party organizations in the South.

Praeger Publishers. 1995. 272 pages. 0-275-94766-1. $55.00

Too Much Liberty?
Perspectives on Freedom
and the American Dream
By David J. Saari

An extended essay on the nature of freedom in contemporary America, its historical roots, and its present-day manifestations.

Praeger Publishers. 1995. 192 pages. 0-275-94879-X. $55.00

Beyond the Rising Sun
Nationalism in Contemporary Japan
By Bruce Stronach

Counters fears of an authoritarian and aggressive Japan, analyzes the ingrained democratic political culture, and explains nationalism as a force in the society today.

Praeger Publishers. 1995. 208 pages. 0-275-95005-0. $59.95

The United Nations and the Maintenance of International Security
A Challenge To Be Met
By James S. Sutterlin
Foreword by Bruce Russett

An insider's view of the changing security functions of the United Nations in the changed conditions of the post-Cold War era.

Praeger Publishers. 1995. 160 pages. 0-275-95052-2. $55.00

Court Reform and Judicial Leadership
Judge George Nicola
and the New Jersey Justice System
By Paul B. Wice

This study examines judicial leadership in court reform, tying biography, political science, social psychology, law, and history into court reform at the local level through the case of a nationally known jurist and reformer—Judge George Nicola of New Jersey.

Praeger Publishers. 1995. 224 pages. 0-275-95038-7. $57.95

GPG-4FAX
1-800-GPG-4FAX
(1-800-474-4329)
For additional information on these titles, or a complete listing of titles in your subject area, call toll-free, 24 hours-a-day.

GPG GREENWOOD PUBLISHING GROUP INC
88 Post Road West, P.O. Box 5007, Westport, CT 06881-5007 • (203) 226-3571 • Office FAX (203) 222-1502

AD95
PLACE YOUR CREDIT CARD ORDER TOLL-FREE, 24 HOURS-A-DAY: 1-800-225-5800

Innis & Ethics

STAPLES, MARKETS, AND CULTURAL CHANGE

Selected Essays

HAROLD A. INNIS
EDITED BY DANIEL DRACHE

An assembly of Innis' most significant and representative writing that have not been readily accessible before.

Innis Centenary Series

Cloth 0-7735-1299-3 $55.00
Paper 0-7735-1302-7 $24.95

UNTHINKING MODERNITY

Innis, McLuhan, and the Frankfurt School

JUDITH STAMPS

The first extended comparison of Canadian and European political thought. Stamps reinterprets the communications theory of Innis and McLuhan as a Canadian variant of the critical theory associated with the early Frankfurt school.

Cloth 0-7735-1232-2 $49.95

CHRISTIAN ETHICS AND POLITICAL ECONOMY IN NORTH AMERICA

A Critical Analysis

P. TRAVIS KROEKER

An analyses of how religio-ethical discourse is changed when it is translated into the economic policy discourse of North American liberalism.

McGill-Queen's Studies in the History of Religion

Cloth 0-7735-1267-5 $44.95
Paper 0-7735-1268-3 $18.95

THE ETHICAL WORLD OF BRITISH MPs

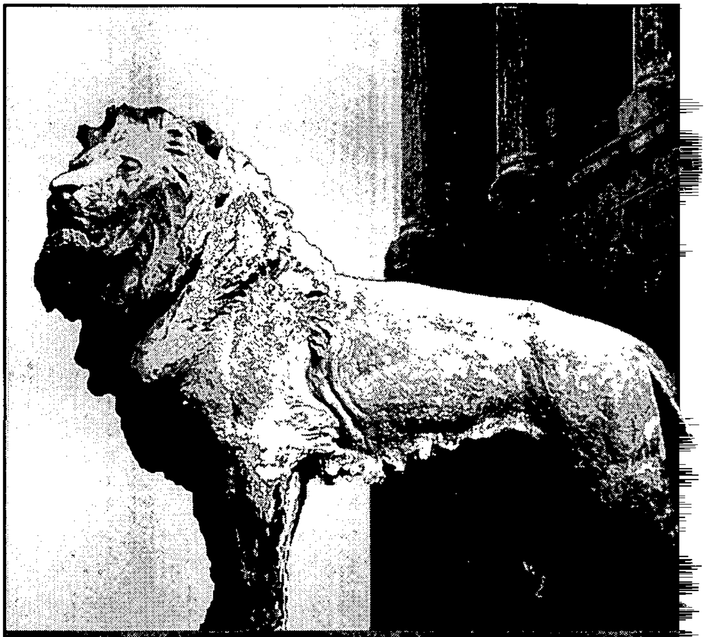
MAUREEN MANCUSO

An examination of the current state of British legislative ethics that raises important theoretical questions about whether the ethical standards of an institution should depend entirely on the personal ethics of individual members.

Cloth 0-7735-1261-6 $44.95

McGILL-QUEEN'S UNIVERSITY PRESS

To order phone: 1-800-565-9523 • VISA/MasterCard accepted

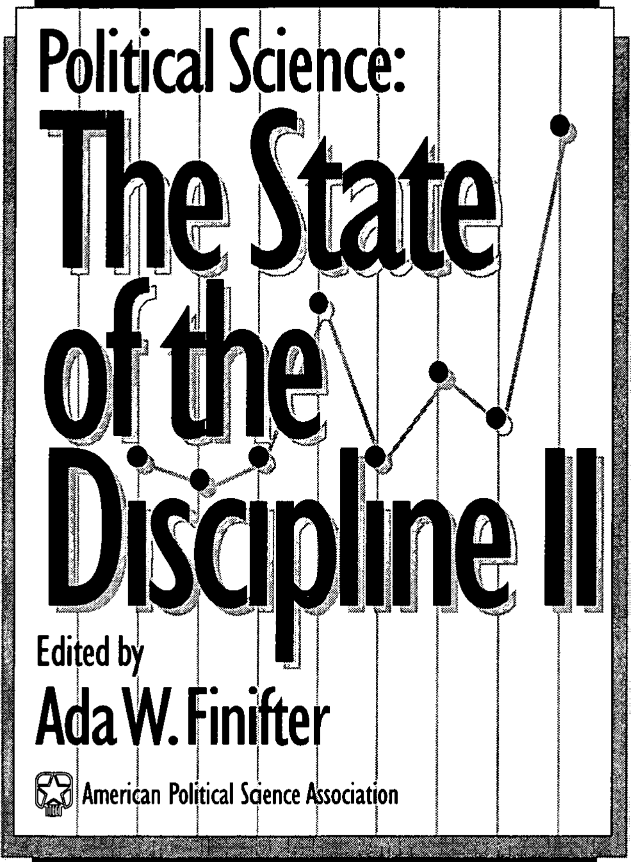


Announcing.

91st Annual Meeting of
The American Political Science Association

August 31-September 3, 1995
Chicago Hilton and Towers
Chicago, Illinois

New Edition...
New Contributors...


Political Science:
The State of the Discipline II
Edited by
Ada W. Finifter
American Political Science Association

Texts and Canons: The Status of the "Great Books" in Political Science
Arlene W. Saxonhouse, University of Michigan

Political Theory in the 1980s: Perplexity Amidst Diversity
William Galston, University of Maryland

Feminist Challenges to Political Science
Susan J. Carroll and Linda M. G. Zerilli, Rutgers University

Formal Rational Choice Theory: A Cumulative Science of Politics
David Lalman, Joe Oppenheimer, and Piotr Swistak, University of Maryland

The Comparative Method
David Collier, University of California-Berkeley

The State of Quantitative Political Methodology
Larry M. Bartels, Princeton University and Henry E. Brady, University of California-Berkeley

Comparative Political Parties: Research and Theory
Kenneth Janda, Northwestern University

The Not So Simple Act of Voting
Russell J. Dalton and Martin P. Wattenberg, University of California-Irvine

The New Look in Public Opinion Research
Paul M. Sniderman, Stanford University

Expanding Disciplinary Boundaries: Black, Latino, and Racial Minority Group Politics in Political Science
Paula D. McClain, University of Virginia and John A. Garcia, University of Arizona

Citizens, Contexts, and Politics
Robert Huckfeldt, Indiana University and John Sprague, Washington University

Political Communication: Scope, Progress, Promise
Doris Graber, University of Illinois at Chicago

Legislatures: Individual Purpose and Institutional Performance
Michael L. Mezey, DePaul University

Public Law and Judicial Politics
Martin Shapiro, University of California-Berkeley

Political Executives and Their Officials
Colin Campbell, S.J., Georgetown University

Public Administration: The State of the Field
Donald F. Kettl, University of Wisconsin

Comparative Politics
Ronald Rogowski, University of California-Los Angeles

Global Political Economy
James A. Caporaso, University of Washington

Political Conflict, War, and Peace
Jacek Kugler, The Claremont Graduate School

ORDER FORM

☐ Please send me ______ copies of **Political Science: The State of the Discipline II.**
☐ My check is enclosed for $ ________.
☐ Charge my: ☐ MasterCard ☐ VISA Exp. date: ______
Card No.: ☐☐☐☐☐☐☐☐☐☐☐☐☐☐☐☐

Copies are $30 for members, $45 for nonmembers.

Add Postage and Handling as follows: $6.00 for U.S. Domestic or International Surface; $7.00 Canada; $18.00 International Airmail
For bulk purchase information, contact APSA.

Orders must be prepaid

Name ______________________
Address ______________________
____
City/State/Zip ______________________
____
Daytime Phone #: ______________________

Return this form with check or credit card order to:

Publications/APSA
1527 New Hampshire Ave., NW
Washington, DC 20036

AMERICAN POLITICAL SCIENCE ASSOCIATION

THE POLITICAL SCIENCE COURSE SYLLABI COLLECTION ORDER FORM

The Course Syllabi Project has developed selected collections of exemplary syllabi for reference and adaption by departments, faculty and graduate students in designing courses. If you are planning a new course, revising a current course, or adding a course to your undergraduate curriculum, the collections of course syllabi edited by experienced faculty provide valuable guidance and references.

INTRODUCTORY/SURVEY COURSES

__ Introduction to International Relations, edited by Linda Brady, Georgia Inst of Technology

__ American Government and Politics, edited by Marjorie R. Hershey, Indiana University

__ Introduction to Political Theory, edited by Peter G. Stillman, Vassar College

__ Introduction to Political Science, edited by John C. Wahlke, University of Arizona

__ Comparative Government and Politics, edited by Frank L. Wilson, Purdue University

ADVANCED/UPPER DIVISION COURSES

__ Public Law, edited by Leif H. Carter, University of Georgia

__ Methodology, edited by John R. Freeman and W. Phillips Shively, University of Minnesota

__ Capstone Course/Senior Seminar, edited by Ronald Kahn, Oberlin College

__ Public Administration, edited by Naomi Lynn, Sangamon State University

__ Political Behavior, edited by Nancy H. Zingale, College of St. Thomas

- Indicate the titles and number of copies desired. The Syllabi are $8.00 EACH.
- Orders must be PREPAID. VISA/MasterCard accepted. Orders shipped via regular UPS.
- Add $3.00 per single copy; $4.00 for orders $10 to $30; $6.00 for orders $30 to $100; and $10.00 for orders $100 to $200.

Name (please print clearly)

Address City State Zip

Method of Payment: __ Check __ VISA __ MasterCard

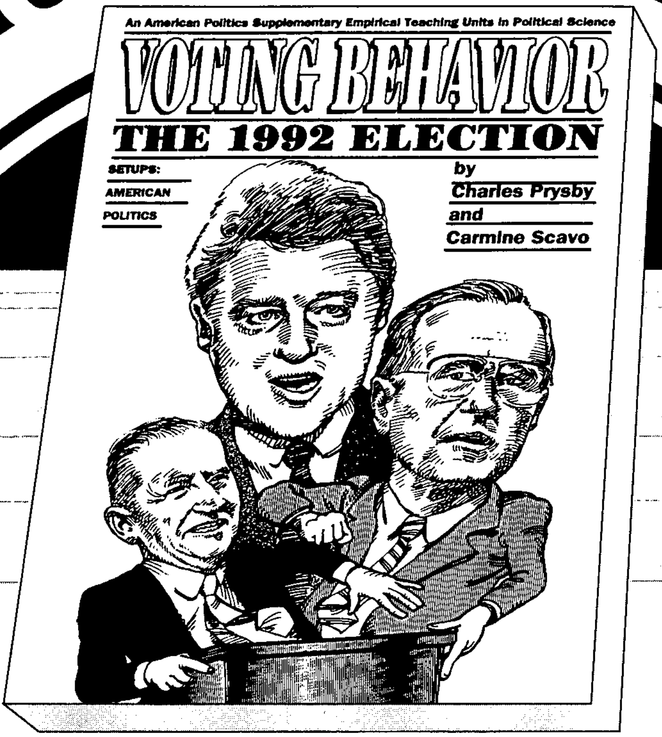
Card Number: ______________________ Exp: _____ Phone: __________

Signature

Send to: APSA Publications, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036
Phone: (202) 483-2512 FAX: (202) 483-2657

NOW AVAILABLE

The SETUPS on the 1992 Election



An American Politics Supplementary Empirical Teaching Units in Political Science

VOTING BEHAVIOR

THE 1992 ELECTION

SETUPS:
AMERICAN
POLITICS

by
Charles Prysby
and
Carmine Scavo

ince the publication of the first SETUPS — for the 1972 Election — Supplementary Teaching Units in Political Science have been widely used in political science courses. Now, there is a SETUPS on the recent presidential election. Voting Behavior: The 1992 Election by Charles Prysby and Carmine Scavo can enable students to learn how to conduct research on voting behavior.

The SETUPS consists of a monograph and a data set. The monograph combines a discussion of the 1992 election campaign and outcome with instructions on how to analyze the voting behavior of the American electorate. Students are able to do secondary analyses using a data set from the 1992 National Election Study. A code book for the data set that includes marginals is in the monograph.

The data set is distributed by the Inter-University Consortium for Political and Social Research (ICPSR), and is available in two versions: for main frame computers or IBM compatible PC's. The data set is sent free of charge for any order of 25 or more copies of the SETUPS. Price: $8.75 each with a 20% discount on bookstore orders of 25 or more copies.

Still available: **Voting Behavior: The 1988 Election**. Use both SETUPS to enable students to compare these national elections in terms of the demographics, party identification, policy, and candidate preferences of American voters. *Order both SETUPS for $13.75*

Order from:
APSA PUBLICATIONS
1527 New Hampshire Avenue, NW
Washington, DC 20036

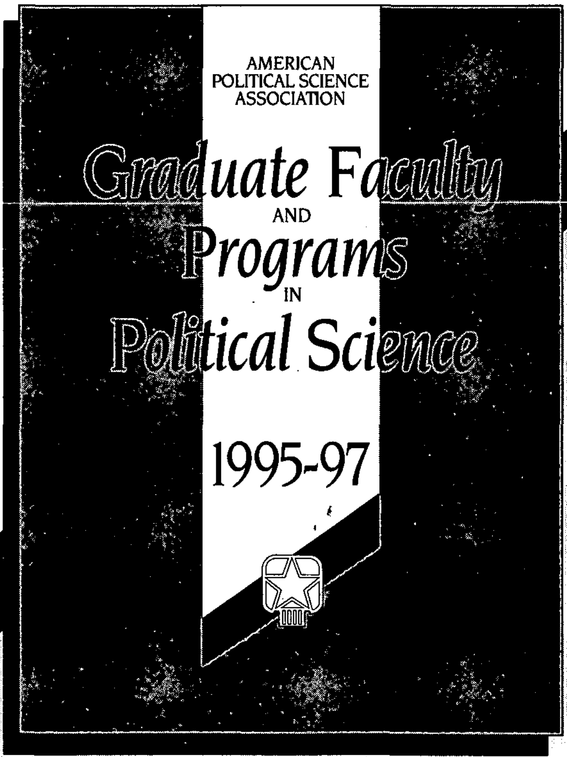
Looking For Graduate Programs and Faculty in Political Science?

The APSA offers **Graduate Faculty and Programs in Political Science, 1995-97**, a comprehensive reference for students, faculty, and college counselors.

Over 350 universities in the United States and Canada describe their Ph.D. and masters programs in political science and associated fields in public policy and administration. Each description includes information about the type of degree offered, tuition charges and financial aid, and admission and degree requirements. Faculty members are listed with their major areas of specialization.

The 1995-97 expanded edition lists more graduate programs, identifies more detailed faculty fields of specialization, and provides e-mail addresses. The directory has an alphabetical index of faculty and a geographical index of institutions. An appendix provides data on each program's ratio of student applications to student admissions, number of degrees awarded, and affirmative action efforts.

Departments that are members of the Departmental Services Program may submit purchase orders. ALL OTHER ORDERS MUST BE PREPAID.



AMERICAN POLITICAL SCIENCE ASSOCIATION

Graduate Faculty AND Programs IN Political Science

1995-97

Order Form

Enclosed is a check or credit card number for _____ copies of ***Graduate Faculty and Programs in Political Science*, 1995-97** at the rate of (check one):

- ☐ $28 + $4 shipping for APSA MEMBERS
- ☐ $20 + $4 shipping for APSA STUDENT MEMBERS
- ☐ $50 + $6 shipping for NONMEMBERS

Name ______________________________

Address ______________________________

City/State/Zip ______________________________

Daytime Phone ______________________________

Indicate: ☐ Visa ☐ MasterCard

Card No. ____________________ Exp. Date __________

Order from: Publications/APSA
1527 New Hampshire Avenue, NW, Washington, DC 20036

CHATHAM HOUSE PUBLISHERS, INC.

Box One, Chatham, NJ 07928 **Phone: (201) 635-2059** **Fax: (201) 635-9366**

AMERICAN DEMOCRACY IN PERIL

Seven Challenges to America's Future

William E. Hudson, *Providence College*

"This text provides a provocative challenge to complacency about the state of American politics. Written with clarity and verve, it will compel students to contest familiar assumptions and to think hard about institutions, social structure, and values." **Ira Katznelson,** *Columbia University*

1-56643-009-7 *paper*

DEMOCRACY'S FEAST

Elections in America

Edited by Herbert F. Weisberg, *The Ohio State University*

Drawing heavily on the National Election Studies, Weisberg and his colleagues have produced invaluable, data-rich analyses of the presidential and congressional elections of 1992. Among the factors introduced and scrutinized are the effects of the Perot candidacy, the unusually visible role of the candidates' wives, and the increase of anti-incumbency and "Congress bashing." Patterns of group voting are analyzed quantitatively and qualitatively. One cannot make sense of the 1994 midterm elections without first understanding what brought Bill Clinton to power.

1-56643-011-9 *paper*

IDEOLOGY AND POLITICAL CHOICE

The Search for Freedom, Justice, and Virtue

Vernon Van Dyke, *University of Iowa*

"This will be an extraordinarily useful book for everyone from novice to political veteran. Van Dyke presents the most important American political ideologies of our time in plain, no-nonsense prose free of rhetoric and cant. His exploration of the central tenets of these ideologies, their historical development, and their implications for practical policy helps make sense of contemporary American politics." **David Menefee-Libey,** *Pomona College*

1-56643-017-8 *paper*

THE POLITICS OF GUN CONTROL

Robert J. Spitzer, *SUNY, College at Cortland*

"This is a book that has been begging to be written, and now Robert Spitzer has filled the void with this insightful volume. *The Politics of Gun Control* is a terrific book—thorough, informative, and balanced. Students will learn a great deal about the policy problems involved and about interest groups, institutions, and the political texture of social regulation."

1-56643-021-6 *paper* 1-56643-022-4 *cloth* **John T. Tierney,** *Boston College*

SEPARATE BUT EQUAL BRANCHES

Congress and the Presidency

Charles O. Jones, *University of Wisconsin—Madison*

"Jones is a rarity: a scholar of American politics who can combine tested theory, practical political savvy, lively writing ability, and balanced insight into one package. Combine that package with his unparalleled understanding of the presidency, Congress, and their interrelationships and you have the essence of *Separate but Equal Branches.* A must for every student of Congress and the presidency." **Norman J. Ornstein,** *American Enterprise Institute*

1-56643-015-1 *paper*

THE REBIRTH OF FEDERALISM

Slouching toward Washington

David B. Walker, *University of Connecticut*

An insightful and provocative assessment of federalism in the United States today. David Walker's analysis and prescriptions are as timely as today's front page. As America's politicians and citizens rethink the proper role and size of government, the division of labor and resources among different levels of government is of prime importance. Walker traces the evolution of American federalism and scrutinizes its current form, calling for a rejection of the current centralized, top-heavy system and a return to a fairer, more balanced system, with revitalized states and localities acting more as equal partners in government.

1-56643-005-4 *paper*

Table of Contents

ARTICLES

A Way of Life and Law
Charles O. Jones ... 1
The Concept of a Critical Realignment, Electoral Behavior, and Political Change
Peter F. Nardulli ... 10
Party Cleavages and Welfare Effort in the American States
Robert D. Brown ... 23
Shaping Mexico's Electoral Arena: The Construction of Partisan Cleavages in the 1988 and 1991 National Elections
Jorge I. Domínguez and James A. McCann ... 34
Modeling Negative Campaigning
Stergios Skaperdas and Bernard Grofman ... 49
Political Control Versus Expertise: Congressional Choices about Administrative Procedures
Kathleen Bawn ... 62
Choosing the Best Social Order: New Principles of Justice and Normative Dimensions of Choice
Grzegorz Lissowski and Piotr Swistak ... 74

RESEARCH NOTES

Economic and Referendum Voting: A Comparison of Gubernatorial and Senatorial Elections
Lonna Rae Atkeson and Randall W. Partin ... 99
Disentangling Patterns of State Debt Financing
James C. Clingermayer and B. Dan Wood ... 108
Avarice and Ambition in Congress: Representatives' Decisions to Run or Retire from the U.S. House
Richard L. Hall and Robert Van Houweling ... 121
Condorcet Winners and the Paradox of Voting: Probability Calculations for Weak Preference Orders
Bradford Jones, Benjamin Radcliff, Charles Taber, and Richard Timpone ... 137

CONTROVERSIES

Civic Virtue and Self-Interest
Christopher M. Duncan; Shelley Burtt ... 147
Aristotle's Science of the Best Regime
Mary P. Nichols; Robert C. Bartlett ... 152

NOTES FROM THE MANAGING EDITOR ... 161

BOOK REVIEWS

Review Essays:

The Civil Culture at Thirty
David D. Laitin ... 168
New Horizons in Institutional Analysis
Elinor Ostrom ... 174
Opinion and Representation
James Stimson ... 179
Political Theory ... 184
American Politics ... 191
Comparative Politics ... 217
International Relations ... 240